THE MATHEMATICAL GAZETTE

EDITED BY
W. J. GREENSTREET, M.A.

WITH THE CO-OPERATION OF
F. S. MACAULAY, M.A., D.Sc., F.R.S.
AND
PROF. E. T. WHITTAKER, Sc.D., F.R.S.

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CONTENTS.

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WILLIAM JOHN GREENSTREET

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"I hold every man a debtor to his profession; from the which as men of course do seek to receive countenance and profit, so ought they of duty to endeavour themselves, by way of amends, to be a help and an ornament thereunto."

THE CONCURRENCE OF TRIADS OF COMMON TANGENTS TO THREE CIRCLES.

By Prof. E. H. Neville, M.A.

"If pairs of common tangents be drawn to three circles, and if one triad of common tangents be concurrent, the other triad will also be concurrent."

This is Casey's enunciation * of a familiar theorem ; Salmon † has the same unqualified assertion, and appends a footnote : "This principle is employed by Steiner in his solution of Malfatti's problem. . . . [His] construction is : Inscribe circles in the triangles formed by each side of the triangle and the two adjacent bisectors of angles ; these circles having three common tangents meeting in a point will have three other common tangents meeting in a point, and these are common tangents to the circles required."

The theorem given by Salmon and Casey is obviously untrue : drawing three concurrent lines and placing circles in the alternate angles, we have the hypothesis without the conclusion. The questions that arise first are: What is the condition that must be added ? Was Steiner, of all geometers, guilty of an oversight ?

To answer the second question we have only to read Steiner's words. Far from relying for his construction on the theorem imputed, Steiner inferred the concurrence of the second common tangents from the construction itself : in relation to the Malfatti circles, these lines are tangents at points of contact, that is, are radical axes, and *therefore*, says Steiner‡, they concur in the radical centre. As is well known, Steiner gave his solution of Malfatti's problem without proof, and it was Hart who reversed the order of deduction and used the concurrence of the lines as a step in demonstrating the construction.

Turning to Hart's paper§, we find the "principle" of which Salmon speaks given as an "evident corollary" to a preliminary lemma. The proofs, of both the lemma and the corollary, are inadequate beyond belief, but Hart does add "It is to be observed, however, that the common tangents should be either all transverse or one transverse and two direct," a sentence of which the existence illustrates the recklessness of Salmon and Casey, since they must have read it. But although it was from Hart that the Irish geometers learned the theorem, and although there is no reason to doubt that he discovered it for himself, Hart was in fact completely anticipated, in the limitation as well as in the theorem, by Quidde, who propounded the results as a Question, elaborately worded, in the *Nouvelles Annales de Mathématique* ‖ in 1852. Quidde's proof is not known, for the Solution published ¶ two years later, which was in the editors' hands in 1853**, was by Mannheim, to whom the theorem has accordingly been ascribed††.

The condition given by Quidde and Hart is, as we shall see, both necessary and sufficient. Nevertheless, it is not easy to conjecture how they came to impose it, unless merely as the result of experimenting with a variety of figures. Carefully expressed, the lemma to which the corollary is attached

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* *Sequel to Euclid*, p. 51 (1st ed. 1881 ; p. 52 in later editions).

† *Conic Sections*, p. 263 (6th ed. 1879). Fiedler reproduces the enunciation and the footnote in his German version of Salmon, p. 425 (4th ed. 1878 ; p. 573 of 6th ed. 1903).

‡ *Ges. Werke*, 1, p. 36. The paper, dated 1826, is reprinted from the first volume of *Crelle*, which appeared in that year.

§ *Q. J. Math.* 1, p. 219 (1857).

‖ Vol. 11, p. 313, Q. 255.

¶ Vol. 13, p. 210.

** Q. 255 is not in the list of Questions still unsolved at the end of vol. 12.

†† In that heavily documented compendium, *Exercices de Géométrie*, prepared by the Institut des Frères des Ecoles Chrétiennes. The reference (p. 326 of the 5th edition), apart from a misprint of 1864 for 1854, which would make Hart the earlier discoverer, is surprisingly careless, for Mannheim in his solution naturally refers to the original question, and there should not be the smallest presumption that a published solution is by the propounder himself. A better justification for the warning to verify every reference would be hard to find.

by Hart is, that if t_{12} is the distance between the points of contact D, E of a common tangent to two circles and if t_1, t_2 are the lengths of the tangents to the circles from a variable point, the locus $t_1+t_2=t_{12}$ consists of the segment DE together with the corresponding segment of the other common tangent of the same kind, while the locus $t_1 \sim t_2=t_{12}$ consists of the parts of these lines which are not between points of contact. On the basis of this lemma, the natural classification of concurrences depends, not on how many of the common tangents are transverse and how many direct, but rather on how many have the point of concurrence between their points of contact and how many do not. A concurrence, in the case of common tangents to three circles, cannot imply a relation of the form $t_{13}=t_{12}+t_{23}$, and therefore would not be expected to imply a second concurrence, unless this relation is the result of an identity of one of the two forms

$$(t_1-t_3)=(t_1-t_2)+(t_2-t_3), \quad (t_1+t_3)=(t_1-t_2)+(t_2+t_3),$$

in which the number of minus signs is odd. We infer that a necessary condition for the application of the lemma is, that the point of concurrence must be outside the segment bounded by the points of contact either on one of the common tangents or on all three of them, and this is the form in which we should have expected the condition originally to be framed.

It is in fact true that Quidde's condition is, somewhat mysteriously, equivalent to the condition just expressed. If, having three circles ρ, σ, τ with common tangent lines u, v, w concurrent in a point O, we replace ρ, which lies in an angle bounded by a half of the line v and a half of the line w, by a circle lying in the adjacent angle bounded by the same half of v and the other half of w, we change simultaneously the type of contact of the line v as a common tangent of ρ and τ and the relation which the segment of w bounded by its points of contact with ρ and σ bears to the point of concurrence O. Thus any change in the figure affects in the same way the parity of the number of transverse tangents and the parity of the number of tangents on which O is not between the points of contact. This establishes the equivalence of the two conditions, without explaining why Hart at least did not express the condition in the form suggested immediately by the manner of his proof.

But if Quidde's condition is seen to be necessary, there is not a shred of evidence in the argument from the relations $t_1 \underset{\sim}{+} t_2=t_{12}$ that it is sufficient. The lack of evidence is proved by the consideration that these relations do not distinguish one common tangent from its companion. If v, v' are corresponding common tangents to ρ and τ, and w, w' corresponding common tangents to ρ and σ, there are four points in which one of the first pair cuts one of the second pair, and we must know something beyond what is common to the lines in a pair, if we are to discover a property of the point (v', w') that is not possessed by the point (v', w). We do not touch the root of this difficulty by dealing with the separate loci $t_1+t_2=t_{12}$, $t_1 \sim t_2=t_{12}$, and so on, and we introduce the further complication that the number of points of intersection of two loci becomes quite uncertain.

With any particular figure it is usually safe to argue directly and so to avoid appealing to a general theorem. This is Coolidge's course * in reproducing Hart's proof of Steiner's construction, and in reality Mannheim's proofs have no better foundation. But in contrast to the inadequacy of Hart and Mannheim and the sketchiness of Salmon is the generality of a characteristic proof of Quidde's theorem given by Baker†, and before concluding that the result never has been established we must examine Baker's argument.

* *Circle and Sphere*, p. 176 (1916). That this cautious author does not mention Hart's theorem confirms my estimate of the unreliability of the traditional proofs.

† *Principles of Geometry*, v. 4, p. 67 (1925).

Let H be any point outside the plane of the three circles ρ, σ, τ, and let Γ be an arbitrary conicoid through H. The cones with H for vertex which contain ρ, σ, τ cut Γ in three conics α, β, γ. Of the two cones through β and γ, only one touches the plane through H and the given common tangent u; this then is a determinate cone U, and similar determinate cones V, W pass through γ and α and through α and β. The three cones U, V, W taken two at a time have common plane sections, namely, the conics α, β, γ, and therefore, the argument continues, there exists a conicoid Σ which these cones all envelop. Since each of the planes through H and one of the lines u, v, w touches one of the cones U, V, W, these three planes, which all contain the line HO, all touch Σ, and therefore HO is a generator of Σ. Hence H is on Σ, and there is a second generator through H. The plane through this second generator which contains the vertex of U touches U and cuts the plane of the original figure in a second common tangent of σ and τ; thus this common tangent u', which is the companion of u, passes through the point Q in which the generator cuts the plane, and so finally the companions of u, v, w all concur in Q.

That this beautiful proof is somewhere defective is evident from the fact that no condition whatever beyond that of concurrence is imposed on u, v, w. The crucial step is the inferring of the existence of the conicoid Σ from the existence of the common sections α, β, γ. This inference cannot be drawn if the cones U, V, W have a common tangent plane, unless their lines of contact with that plane concur, nor can it be drawn if the vertices of the cones are collinear; the question it becomes necessary to discuss is whether these exceptional cases can be dismissed as infinitely unlikely or whether in the circumstances they ought to be taken into account.

If the vertices of the cones are collinear, the points in which the lines joining these vertices to H cut the plane of the original figure are collinear also. But the point in which the line joining H to the vertex of U cuts the plane is the point in which the tangent u cuts its companion u', or, in other words, is that one of the two centres of similarity of ρ and τ which lies on u. Now we know that the six centres of similarity of three circles lie by threes on four lines: they are the vertices of a quadrilateral. The three external centres are collinear, and the external centre of each pair is collinear with the internal centres of the other two pairs, but the three internal centres are not collinear, nor can an internal centre be collinear with two external centres. Hence if the tangents u, v, w are all transverse, or if one of them is transverse and the other two are direct, the centres of similarity which lie on these tangents are not collinear; thus if Quidde's condition is satisfied, the vertices of the cones U, V, W cannot be collinear.

The connection between Quidde's condition and the collinearity of vertices becomes even clearer if we direct our attention to a figure in space constructed from the conics α, β, γ. Through the two conics β, γ there pass two cones U, U'. The line joining the vertices of these two cones is the polar for the conicoid Γ of the line of intersection of the planes of β and γ. It follows that if the planes of the three conics α, β, γ meet in Z, the vertices of the two cones through β and γ, the two cones through γ and α, and the two cones through α and β, all lie in the polar plane of Z for Γ. Moreover, if this plane cuts α in A_1, A_2, cuts β in B_1, B_2, and cuts γ in C_1, C_2, Pascal's theorem, applied to the six points A_1, B_1, C_1, A_2, B_2, C_2, which all lie on the section of Γ by the plane, implies immediately that the vertices of the six cones lie by threes on four lines: these vertices are the vertices of a quadrilateral, of which the quadrilateral whose vertices are the centres of similarity of ρ, σ, τ is merely a projection from H. Not only is it impossible for the vertices of U, V, W to be collinear if Quidde's condition is satisfied, but these vertices necessarily are collinear if the condition is not satisfied: collinearity is not an exception to be ignored in a general treatment, but a configuration that

occurs in four of the eight selections that can be made from the pairs of cones U, U'; V, V'; W, W'.

Supposing that Quidde's condition is satisfied, we have still to consider the possibility of a common tangent plane to the cones U, V, W. Such a plane must of course be the plane through the vertices of the cones, and therefore must be the polar plane of Z for the conicoid Γ. If this plane touches the cones U, V, W, it touches the conics α, β, γ; hence the polars of Z for these conics are tangents to the conics. Thus if this case occurs, Z is a point common to the three conics; the plane containing the vertices of U, V, W becomes the tangent plane to Γ at Z, and its lines of contact with the cones all pass through Z. It follows that the existence of the common tangent plane does not invalidate the proof of the existence of the enveloped conicoid Σ, and therefore that Quidde's condition is sufficient as well as necessary for the validity of Baker's proof. Hence Quidde's condition is sufficient for the truth of the theorem. It remains to ask whether it is necessary to the result, or only to this particular proof.

Returning to the original plane figure, consider ρ and σ as fixed circles and τ as a variable circle touching the fixed lines v, u, of which v is a tangent to ρ and u a tangent to σ. The common tangent w of the circles ρ and σ cuts its companion w' in a fixed point N which is one of the centres of similarity of the circles ρ, σ. If the variable companion u' of u cuts u in L and the variable companion v' of v cuts v in M, Quidde's condition is, that L and M are not collinear with N. But the line LM necessarily passes through one or other of the centres of similarity of ρ and σ, and therefore Quidde's condition is, that LM passes through the second centre of similarity of these circles, which is a definite fixed point N'. If A, B, C are the centres of ρ, σ, τ, when Quidde's condition is satisfied, the triangle LMC has its sides passing through fixed collinear points A, B, N' and has the vertices L, M moving on fixed lines u, v; hence the locus of C is a single line, which is of course a definite one of the two bisectors of angles between u and v. The locus of the centre of τ when Quidde's condition is not satisfied is the other bisector of the same angles.

Now let Q be any point on w', the companion of w, and let v', u' be the second tangents from Q to ρ, σ, cutting v, u in M, L. The line LA cuts the bisector for which Quidde's condition is satisfied in a definite point C, and since Quidde's condition is sufficient, the circle with centre C which touches u and v touches v' also and has its centre on the line MB. But there cannot be more than one circle which has its centre at the intersection of LA and MB and touches one of the lines u, v, u', v', and therefore there cannot be in addition to the circle of whose existence we know, for which Quidde's condition is satisfied, a second circle for which the condition is not satisfied: the condition is necessary as well as sufficient.

If three circles ρ, σ, τ in a plane are such that a common tangent to σ and τ, a common tangent to τ and ρ, and a common tangent to ρ and σ are concurrent, then either the three points in which these common tangents meet their companions are collinear, or the three companions also are concurrent; the alternatives are mutually exclusive.

Literally, the first alternative occurs when the number of direct common tangents is odd, the second when the number of transverse common tangents is odd. But the enunciation adopted remains adequate if the circles are replaced by any three conics with a pair of common points, when the distinction between direct and transverse may be irrelevant even in cases in which it can be said to survive.

E. H. NEVILLE.

EUCLID (I. 4) AND TIME-SPACE THEORY.

By E. T. Dixon, M.A.

Dr. Robb, in his rejoinder to my criticism of his paper, asks me a direct question ; and as, to me at least, the matter under discussion is of far greater importance than any mere difference of opinion between us, I hope I may be permitted to reply.

Dr. Robb after quoting a paragraph from my paper, goes on to quote from his own work *Theory of Time and Space,* where, in defining " normality in the different types of line," he says : " Only one case will be found to be strictly analogous to the normality of intersecting straight lines in ordinary geometry . . . ," while of another case he goes on to say, " the use of the words ' at right angles ' would, in this case, clearly be an abuse of language." He then asks his question : " In the face of this passage does Mr. Dixon still imagine that I mentally substituted for (my) term ' normal ' an Euclidean term, such as ' at right angles ' ? "

My answer is : Certainly I do. What I had written was not exactly that I " imagined " that Dr. Robb has made a mental substitution but—" it must therefore be taken that " he had done so. I purposely used words which I hoped would not suggest any doubt as to the clarity of Dr. Robb's own thinking, or the symbolic correctness of the definitions in his own *Theory,* which I expressly admitted. But now that he asks me, I of course admit that I did imagine that he had made a mental substitution, when he passed from symbolic reasoning to its application to Euclid's geometry, and I see nothing whatever in Dr. Robb's rejoinder to induce me now to imagine anything else, for there is in it nothing materially new to me. He went on, in his original paper, to do the very thing he in his rejoinder describes as an " abuse of language," and which I described in scholastic terms as a " fallacy of the ambiguous middle." When he passed on to compare his own theory with Euclid's I. 4, he swooped over from his own symbolic definition, and perhaps from his own criterion of equality, to that of Euclid. In I. 4 Euclid does not say anything about right angles, he only says the angles between the sides of the two triangles are given as " equal " ; I am therefore justified in taking it that Dr. Robb in applying his theorem passed not only from his definition of normality to Euclid's conception of equality, but to his conception, or axiom, that all right angles are equal to one another. It would be just as logical for me to quote Dr. Robb's theorem as a confirmation of Euclid's theorem III. 31, that the angle in a semicircle is a right angle ; with which it has indeed just the same sort of analogy, and which might as easily lead to just the same sort of ambiguity.

There is, I repeat, nothing materially new to me in Dr. Robb's rejoinder, interesting as it is to me for the purpose of trying to follow more closely his line of thought. But there is one point at least which puzzles me still ; I cannot see why he quoted from Sir T. L. Heath's monumental work. Surely he cannot have been thinking of settling the matter by an appeal to Authority, however great that of Sir T. L. Heath may be ? I am ashamed to say that I have never even heard of him or his work, which, if I were claiming to write with authority myself, would of course put me out of court at once. But, so far as Dr. Robb's quotations from his work go at any rate, I can find no discrepancy between what he writes and anything in my paper ; nothing to suggest that his line of thought may not be exactly parallel to my own. Of course everybody can see that Euclid tried to make as little use as possible of the conception of superposition. Perhaps even it would have been as much as his place was worth to do otherwise ; his merit is, however, that he had the courage of his opinions, and did use it, where necessary. No doubt he, in his day, was up against the scholastic powers that then were, as Francis Bacon was in his day, but could not, like the latter, offer them a pitched battle.

Unfortunately that battle at best resulted only in a draw; the powers that be are still under the thraldom of what he called *Idola specus*, the idolatry of Aristotelian logic; they are still striving, indeed more than ever, to eject anything like doing, efficient causation, free will, choice, or even "passing in review"; from philosophy, and even from mathematics.

But obviously it is impossible to attempt to suggest my own line of thought in a short article, about that of Dr. Robb. For that reason I must say that it was hardly fair of Dr. Robb to suggest that I had been "falling into the evil ways" of the pure mathematicians, and that I had attributed any such "evil ways" to them, by saying that, "as such" pure mathematicians did not trouble themselves much about the real import of their symbols; or to criticise me for not ascribing any "definite meaning to that *constancy of magnitude* which the method of superposition presupposes." Dr. Robb left out the last half of my sentence about "mathematicians as such," which made it clear that the same men, in other capacities, did trouble themselves, to some extent; even though it might be more fortunate for us if they did so more often. As a whole therefore the sentence excused, rather than blamed, mathematicians. And if Dr. Robb were able to follow my line of thought more closely he would find that I regard mathematicians, at least the greatest among them, like Euclid, Newton, Rowan Hamilton, Clerk Maxwell, as just the people who have in practice wrenched themselves free from the Aristotelian thraldom, even if perhaps certain others, say Leibnitz, Sophus Lie, Cantor, and perhaps Bertrand Russell, are still bound by it (though for the latter I have hopes).

But of course Dr. Robb cannot expect me to suggest my line of thought on so vast a subject in a brief article. I am now trying to do so in a "monumental work," which I am hoping some day to get published. If, or when, that happens I can only hope that Dr. Robb will not find it too long, or too dull, to read.

EDWARD T. DIXON.

REPLY BY ALFRED A. ROBB, F.R.S.

IN spite of my explanation of what I really did mean, Mr. Dixon appears still to imagine that I "mentally substituted for (my) term 'normal' an Euclidean term such as 'at right angles,'" and he ignores what I pointed out, namely, that the actual words which I used were: "so that the *analogue* of Euclid (I. 4) does not hold in this case."

If I had considered the case where the two given sides are normal to one another as being equivalent to one where they are "at right angles," why should I have gone on to consider a more complicated example, since my point would have equally well been made by the simplest case available?

Although, in my former reply, I called Mr. Dixon's attention to the fact that I had gone on to consider the case where the two given sides are both segments of inertia lines of lengths b and c and making a finite hyperbolic angle with one another equal to $\log \frac{b}{c}$, yet he persists in ignoring this and harping on his original objection.

If Mr. Dixon is so fastidious in his language as to object to my use of the words: "so that the analogue of Euclid (I. 4) does not hold in this case," let him omit them altogether and read on, and he will find that my argument is not affected by the omission.

I should like to recall to Mr. Dixon a passage from his original attack on my paper. Mr. Dixon says: "Nobody can transfer an idea from his own mind to that of another man, in the sort of sense in which he might transfuse some of his blood into his veins; he can at best only act in such a way as to suggest to the other man a train of thought which that other man has to work out for himself."

I am glad to find myself in substantial agreement with Mr. Dixon on this point, but I should like to add that the difficulty of carrying out this process is much enhanced if there be not a certain modicum of good will evinced by the recipient.

However, the following remarks may help to make my position clear and to show why I introduced the case where the two given sides are " normal " to one another.

In building up Time-Space geometry from the *before* and *after* relations, the ideas involved in what I have called " normality " are logically prior to the definitions of *congruence*, of angles in general.

This is the case both in an (x, y) plane and in an (x, t) plane.

Taking the congruence of lengths as already defined, then the congruence of angles, circular or hyperbolic, may be defined in the pure mathematical sense by means of the inverse circular or hyperbolic functions, provided that we have already defined " normality."

The definitions both of " normality " and of the congruence of lengths have already been given in my *Theory of Time and Space.*

Accordingly, in my original paper in the *Mathematical Gazette* I first considered the case of the " normality " of a separation line to an inertia line, and then showed in what sense two inertia lines could be regarded as being at a given finite hyperbolic angle to one another.

With the resulting meaning of these lines being at a given hyperbolic angle, I showed that the analogue of Euclid (I. 4) did not hold in all cases.

To have done this without incidentally mentioning that *the analogue such as it is* also breaks down in a similar way in certain cases in an (x, t) plane when the given sides are normal to one another, could hardly be excused on the ground that the lines would then be of different types and the analogue less close. On the one hand, if I had not mentioned it, sympathetic readers of my article might have regarded the omission as a fault; while, on the other hand, Mr. Dixon would have been deprived of something to grumble at.

This would have meant a loss of happiness all round.

But now while we are on the subject of grumbling: Mr. Dixon says that it was hardly fair of me to criticise him for not ascribing any " definite meaning to that *constancy of magnitude* which the method of superposition presupposes." Mr. Dixon says: " But obviously it is impossible to attempt to suggest my own line of thought in a short article about that of Dr. Robb " ; while further on he says: " I am now trying to do so in a ' monumental work ' which I am hoping some day to get published." Thus we appear to have reached a most curious state of affairs. According to Mr. Dixon the method of superposition is a valid method of geometrical proof and, presumably, always has been from the time of Euclid onwards; but this palpable objection to it will only be answered, and the cogency of the method will only become patent to the world, when Mr. Dixon's book appears.

Again Mr. Dixon says: " But there is one point at least which puzzles me still; I cannot see why he quoted from Sir T. L. Heath's monumental work. Surely he cannot have been thinking of settling the matter by an appeal to Authority, however great that of Sir T. L. Heath may be ? "

In reply to this I may say that I regard any man as *speaking with authority* on a mathematical subject when he puts forward an argument which I find convincing and have no ground to doubt. The same man, on another occasion, may put forward another argument to which I can raise objections which he cannot refute, and in that case I do not regard him as *speaking with authority*, however great he may be in other respects.

It is quite possible in the former case that we may both have been mistaken, while in the latter case it is possible that I may have misunderstood him: for I have not yet come across any man who is infallible in all things. When, therefore, I referred to Sir T. L. Heath's work I did so because his edition of

Euclid (issued by the Cambridge University Press in 1908 in three large volumes) is, I believe, by far the best and most elaborate modern edition of Euclid which has been published in the English language, and contains, in a conveniently accessible form, various arguments which have been put forward from time to time by different writers against the method of superposition. Some of these may appeal to one person and some to another, but, as I could not take up the space of the *Mathematical Gazette* in repeating them all, I contented myself by quoting a few short passages and referring Mr. Dixon to the work itself for the rest.

Mr. Dixon will thus observe that the answer to what he finds so puzzling is quite simple when looked at in the proper way. A. A. Robb.

[This discussion is now closed.—*Editor.*]

THE PRESIDENT'S NEW TITLE.

Members of the Association have the pleasure of congratulating their President, Prof. Sir A. S. Eddington, on the Knighthood bestowed on him in recognition of his services in the promotion of astronomical science.

British Association.

BRISTOL, SEPTEMBER 3-10.

For one reason and another, mathematicians have taken little part in the last two meetings of the British Association, but this year the opportunity for a vigorous subsection recurs, and there is to be a programme of mathematics covering at least three mornings.

Prof. A. C. Dixon will give an account of the development of the subject of integral equations; shorter papers on analysis will be given by Prof. Berwick, Mr. Bosanquet, Mr. Chaundy, Mr. Linfoot, Prof. Titchmarsh, Dr. Wrinch, and Miss Young. Papers on geometry are promised by Mr. Coxeter, Mr. Du Val, Mr. Hodge, Mr. Richmond, and Mr. L. C. Young.

The common interests which bring Dr. Comrie, Mr. Irwin, and Mr. Wishart together on the Tables Committee of the British Association will bring them into sequence on the time-table. Prof. Brodetsky will apply his talent for exposition to the latest Einstein field-theory, Prof. Daniell whets curiosity with the title "The mathematical theory of flame motion," and it is safe to assume that Dr. R. A. Fisher will not speak on "Inverse probability" without finding something enjoyably provocative to say.

The subsection will not have a complete monopoly of mathematics at the meeting, but how far any particular astronomer or physicist will talk mathematically on any particular occasion is hard to foretell, and a list of attractive names which would be easy to begin would be difficult to bring to its invidious end.

Mathematicians have never doubted the pleasure of afternoons and evenings at the meetings of the British Association, and it is to be hoped that a year in which the mornings also should entice them will see them present in large numbers.

GLEANINGS FAR AND NEAR.

768. Dr. Hutton, in his treatise on mensuration, p. 119, says, "As the *famous* quadrature of the late Mr. John Machin . . . is extremely expeditious and *but little known*. . . .—R. L. and Maria Edgeworth, *Essay on Irish Bulls*, 3rd edit., 1808, p. 78.

THE STORY OF A PROBLEM AND ITS SOLUTION.*

By The Rev. J. J. Milne, M.A.

When I began to consider how I could best fulfil my promise to read you a paper, my difficulty in selecting a subject arose, not from the dearth of matter, but from the wide range before me, as I was told I might select any mathematical subject I liked. The subject of mathematics as a whole I at once put aside, as I felt it would be impossible for me in a short paper to say anything new about it. For the same reason my favourite subject geometry was unsuitable, and as I was brought up on the Elements of Euclid, anything I might have to say about geometry would probably be considered old-fashioned and antiquated. Then I thought of the subdivision Geometrical Conics, which has always been my special study, but here again the subject is too extensive, and I am told that at present it is rather out-of-date and often does not find a place in the ordinary school curriculum. Still using the pruning knife, I thought of speaking to you about a certain celebrated problem and its ramifications in higher geometry, of which it is practically the foundation stone. Here again I was afraid its demonstration and treatment might seem too technical for those of my hearers who had not made a special study of Geometrical Conics, or whose knowledge of it was a little rusty, but I felt that at any rate the story connected with it would interest every student, whether of mathematics or classics or any other branch of education.

About 240 B.C. there was born at Perga in Pamphylia a Greek mathematician named Apollonius. His chief work was a treatise on Geometrical Conics. He lived about seventy years after Euclid, and the difference between their treatises was this. Euclid found a large amount of geometrical properties already well known, and these he arranged in a logical order, without necessarily having been the discoverer of any of them. Apollonius, on the other hand, found a comparatively small number of properties of conics were then known, and he practically discovered almost all there was remaining to be known of the subject. In my own mind I often compare his work to a quarry, the existence of which is indicated by a few outstanding blocks of stone, and from which, after a certain amount has been taken, the quarry is to a great extent worked out. So it was in the case of Apollonius. He found a few known conic properties, and these led him to the subject which he systematically attacked and worked out, and the treatise earned him the title of "the great geometer". His treatise consisted of eight books, or, as we should call them, chapters, containing 487 theorems. The first four books are extant in Greek, but the last four disappeared for a time, and the story of their recovery is interesting.

In 1661 Alphonsus Borellus, Professor of Mathematics at Pisa, was spending his holidays in the north of Africa, and at one of the monasteries where he was staying the monks showed him the manuscripts in their library, and amongst them was one in which he recognised the diagrams of the first four books of Apollonius from which he had learnt his conics, and there were a great many more figures which were new to him, and he at once felt that these were in all probability the books which had been lost. It turned out that they were books 5, 6 and 7, but the text was in Arabic, a language with which he was unacquainted. He called to his assistance his friend, Abraham Echellensis, Professor of Oriental Languages at Florence. Their task was not an easy one, as the copy before them had been made by a scribe who evidently did not know Arabic, for the diacritical points were wanting, so that the letters were, as it were, mere matter without form. Also to save himself trouble the scribe had made some of the figures do duty for many propositions, so that they seemed hopelessly confused. However, between them, they managed in three months to complete their version and translate it into Latin.

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* A paper read before the Portsmouth Mathematical Society, 8th April, 1930.

The eighth book was not in the manuscript, and is still missing. Now Apollonius in his general preface says that in the third book he had given a complete solution of a certain problem. He does not say what the problem was about, but merely mentions its name, the "locus ad tres et quatuor lineas". All he told us about it was that Euclid was able to solve it only in a special case, and that it was not possible to solve it completely without the aid of certain properties which he, Apollonius, had discovered.

The next mention we hear of the problem is 700 years afterwards, by Pappus, a Greek mathematician who lived about A.D. 400. He rather discredited Apollonius' statement, and though he did not doubt that he had obtained a solution which was lost, and not to be found in his works, he seems to imply that Apollonius had got it from one of Euclid's pupils.

From Pappus we learn for the first time what the problem was, viz. Given a quadrilateral and a moving point P in its plane and straight lines drawn from P to the sides of the quadrilateral making constant angles with them, then if α, β, γ, δ are the lengths of these lines taken in order, and if the product $\alpha \,.\, \gamma$ is in a constant ratio to the product $\beta \,.\, \delta$ the locus of P is a conic passing through the angular points of the quadrilateral, with a suitable modification in the case of a triangle.

We hear no more of the problem until 1633, when Pappus' statement attracted the attention of the French philosopher and mathematician, René Descartes, who spent about six weeks in vain attempts to solve it geometrically, and being determined to obtain a solution he invented the system of geometry which is now called analytical, and starting with this problem as his basis he wrote his celebrated *Geometry*, which was published in 1637.

The next person to attack the problem was Sir Isaac Newton, *c.* 1700. In his *Principia*, when he came to consider the case of a planet describing an orbit satisfying five conditions, neither of the foci being given, this problem was one of those which presented itself, and as the treatment of the *Principia* was entirely geometrical, it was necessary for him to obtain a geometrical solution of the problem. This he succeeded in doing, and took great credit to himself for having been the first to do so; and he indulged in a gentle hit at Descartes because he was only able to solve it analytically.

We now come to our own time. In 1893, in collaboration with my old friend R. F. Davis, now alas passed away, I wrote a book on Geometrical Conics; one of our objects was to show what propositions were due to Apollonius, and what were of recent discovery, our intention being to conclude the book with the celebrated problem as a sort of coping stone, and this portion of the book was part of the task assigned to me. Now if you consider the title of the problem, "locus ad tres et quatuor lineas," it is obvious that in seeking a solution you could either first prove it for the triangle, and then deduce it for the quadrilateral, or you could begin with the quadrilateral and deduce the case for the triangle, and the latter was the method adopted by Newton; but it always appeared to me that if this had been the method employed by Apollonius he would have spoken of the problem as the "locus ad quatuor et tres lineas," and not as the "locus ad tres et quatuor lineas"; and that just as in mountaineering there are in general more than one distinct routes to the summit, I felt convinced that there was another solution to be obtained by approaching the problem through the triangle, and that although Newton's solution was strictly geometrical, I did not think it was the solution which Apollonius had obtained, and that there must be a different solution if only it could be found, and it was my job to find it.

The rest of our book was completed, and the printers were becoming impatient for the rest of the manuscript, and the weeks passed by, but still there was no solution forthcoming, and although the problem was seldom out of my mind, whether awake or asleep, I could not see daylight. At the time I was living at Dulwich, within an easy walk of the Crystal Palace, where I often

used to go on a Saturday to the popular concerts, and one Saturday afternoon I was, as I thought, listening to the strains of the grand organ pealing through the building when suddenly my subconscious mind, which evidently, unknown to myself, had been working at the problem and been stimulated by the sound of the music, suggested to me to try a method and employ a property which had previously escaped my notice. I at once left the concert and went home, and in a few hours, following the path pointed out to me, I obtained the long-sought solution. This enabled us to finish our book, in which we repeated the statement made by all previous writers that the solution of Apollonius was lost, but in giving Newton's solution and my own, I ventured to say that if ever the lost solution obtained by the great geometer was found the method would probably be more in accordance with mine than with that of Newton.

The end of my story is now in sight. Our book appeared in 1894. A few months later I was asked to write a paper for the Christmas meeting of the Association for the Improvement of Geometrical Teaching, now called the Mathematical Association. I was told that Dr. Taylor, the late master of my own college, had promised to read one on the modern geometry of conics, and as the subject was very fresh in my mind I promised to read a paper on the conics of Apollonius. I proposed to give a brief summary of each of the seven surviving books. When I came to the third book in which in his general preface Apollonius distinctly stated that he had obtained a solution of the problem, at the end of the book were three propositions, very long, and of a very complicated nature. In my previous study of Apollonius I had often read them through, but I never could make out what the writer was driving at. They covered several pages of Greek text, and led to results involving the ratio of the products of quantities of the sixth degree, and I had always left them severely alone. Now, however, I felt that I must say something about them in my paper, and after reading through them again, and feeling more befogged than ever, the idea suddenly occurred to me that possibly they might be the tomb in which the lost solution was enshrined. With this idea in my mind I set to work to reduce and simplify the very forbidding-looking results, and in a short time I was able to lay bare the missing solution in all its simple beauty after it had lain hidden there for more than 2000 years. What pleased me very much was to find that, as I had anticipated, it was through the triangle, not through the quadrilateral, that he had obtained his solution.

There are two points connected with the problem which are difficult of explanation.

(1) Although from the problem itself can at once be derived the cross-ratio property of points on a conic, the theorems of Pascal, Desargues, Maclaurin, and other properties of higher geometry of which it may be called the *fons et origo*, yet it was proposed as a matter for discussion in the infancy of the subject by a mathematician whose name has not come down to us.

(2) Although the solution was given in the three propositions at the end of book 3 of Apollonius, a book which was always extant in Greek and was used as the textbook from which the subject was learnt, and was therefore in the hands of all students of conics, two of whom, viz. Descartes and Newton were extremely desirous of finding a solution, yet it does not seem to have occurred to any one to look for it in the place where Apollonius said it was to be found.

The form in which Apollonius left it with its reduction to that with which we are now familiar, together with Newton's solution, are given on pp. 102-108 of *Isaac Newton, 1642-1727* (Bell & Sons. 1927).

The solution of Descartes by analysis will be found in the *Mathematical Gazette* for April 1929. JOHN J. MILNE.

MATHEMATICS IN THE FIRST SCHOOL CERTIFICATE.

A Discussion at the London Branch.

A Meeting of the London Branch of the Mathematical Association was held at Bedford College, Regent's Park, London, N.W., on Saturday, 22nd March, 1930, for the purpose of discussing "What Changes, if any, are advisable in the Elementary Mathematical Syllabus of the First School Certificate ?" Mr. J. Katz presided.

The **Chairman,** in calling upon Mr. Paterson to open the discussion, said that he had much pleasure in presiding over such a thoroughly representative gathering to consider a subject of great importance. The meeting was fortunate in that Mr. Paterson was not only a teacher but an examiner of wide experience. The speaker hastened, however, to assure Mr. Paterson that those present had not come either to bait examiners or to ventilate their grievances with regard to any particular examination question, but to re-open the whole question as to what was the most suitable syllabus in Mathematics for the First School Leaving Certificate.

Many, especially hard-working administrators who had to draw up examination regulations, would no doubt regard the revision of syllabuses as reprehensible, but it had to be remembered that in the majority of schools the syllabus of the examination largely determined the curriculum in Mathematics. He had no doubt that those responsible for the original drawing up of the syllabus of any particular examining body had in mind a certain curriculum ; they had, in fact, a set of educational values with regard to different parts of Elementary Mathematics. Notoriously, opinion changed, and ought to change, as to what is important and what is relatively unimportant in Elementary Mathematics. Only a year or two ago it had been decided that logarithms were sufficiently important to be inserted into the London syllabus. After all, what was examined on was necessarily determined by what was taught : the two had a complementary relationship. There were, of course, other examining bodies than that of London, and it was good that those familiar with the syllabuses of various examining bodies should meet together, and compare ideas and thus form a critical body of opinion as to what, in the present state of mathematical teaching, should be admitted to the syllabuses and what should be omitted.

Mr. **W. E. Paterson** then opened the discussion as follows :—

I think it is about twenty years since the Board of Education published *Regulations for the First School Examination.* I have never seen those *Regulations,* but I have always understood that the idea was that any syllabuses suggested should be suitable for the normal boy or girl of sixteen ; and in discussing what changes are necessary I think we ought to bear in mind that the syllabus is meant to cater not for the pupil who is bright in Mathematics, but simply for the pupil rather below the normal, because in most examinations Mathematics is almost compulsory on every candidate, and so you have brilliant Classical and Modern Language candidates forced to take Mathematics. That must be borne in mind when suggesting that the syllabus should be altered, particularly if altering means widening.

There is also the bugbear in that when talking of the School Certificate many mean Matriculation ; that is to say, the Credit Standard of the School Certificate. In reality, the General School Certificate has quite a low standard for passing, whereas Matriculation requires a higher standard.

Complaints or suggestions about a syllabus are, I suppose, either on the score that the present syllabus is too hard or that it is too easy, or that the modern teacher feels himself cramped because he is confined within rather old-fashioned limits and thinks he could teach better, and do his pupils more good, if changes were made and something new put in. I do not suppose any

of those present profess to know the *Regulations* for all the six or seven School Certificate Examinations in England. This being the London Branch, I take it the majority are concerned with London *Regulations*.

London deals with the complaint that the syllabus is too hard by saying that " In Elementary Mathematics . . . a somewhat lower standard may be accepted for the purposes of Matriculation, provided that the candidate has also reached the required standard in five other subjects." * Thus if anybody objects to the present syllabus because it is too much for the Classical and Modern Language candidates, there is an allowance so far as reaching Matriculation standard is concerned.

Those who complain that they are cramped in their teaching because the syllabus is old-fashioned are also catered for in London, the *Regulations* stating that " papers may be set more closely in accordance with the School Curriculum, provided that any syllabus proposed is submitted † by a certain date." Thus London University has declared its willingness to examine any school on its own syllabus.

Another complaint is that, possibly, the syllabus is too easy. In 1929 55% obtained Credit standard, and in 1928 I think 50% obtained it. The figure is generally of the same order, so that it does not seem that the syllabus is too easy. In fact, so far as London is concerned all three classes of objectors are catered for. Still, I suppose we have got to go on with the discussion !

Taking the syllabus and coming to Arithmetic first, the London School Certificate has one marked peculiarity: there is an arithmetic paper, and then we have arithmetic combined with algebra in another paper. I think that is one of the points for discussion. Is there any reason for this duplication of arithmetic ? Nearly all the other School Certificate Examinations have the three papers, Arithmetic, Algebra and Geometry, and the Oxford and Cambridge Joint Board has, I believe, all three mixed in three different papers.

As to logarithms, it is well to remember that the use of logarithms is *permitted* in arithmetic ; it is not compulsory. Presumably the examiners take it that any questions set in the arithmetic paper should be capable of being done without logarithms. It is important to remember this. There is a remark I came across in the Cambridge School Certificate Regulations, or the Senior Local as it used to be called, in reference to logarithms which I should like to see repeated in all the *Regulations*, viz. " Tables of four-figure logarithms of numbers will be printed on the back of the question paper in Arithmetic. Candidates are advised not to use them in attempting to solve any question unless they are satisfied that the answer can be obtained to a sufficient degree of accuracy." It is important to bear that in mind and I think the Cambridge people are wise to have put it in their *Regulations*.

Another point that arises in connection with Arithmetic is the reference to Mensuration. London as far as arithmetic itself is concerned—not arithmetic and algebra—is very vague on the subject of mensuration. It is simply " easy mensuration " without any detail. The Cambridge Local is much better: " Questions will be set on elementary mensuration ; these may involve the use of formulae for the right-angled triangle, circle, cylinder, cone, sphere, right prism, pyramid. Candidates will be expected to give from memory only the formulae for the triangle and the circle." I think that here is a point we may wish to say something on: how far mensuration should be brought distinctly into the Arithmetic syllabus. Durham simply says "areas and volumes," which might cover all mensuration or nothing at all.

Some syllabuses tell you what is not in them, and that is rather useful. Durham says that questions will not be set on " recurring decimals, Troy weight, Apothecaries' weight, true discount," and so on.

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* *Regulations for Inspection and Examination of Schools*, 1930, p. 214.

† *Ibid.* p. 214.

Looking through the algebra syllabuses I could not find very much for comment except perhaps the question of the gradient of a graph. There is very little difference between the syllabuses of Durham, Cambridge and London.

As regards Geometry, London differs from nearly every other School Certificate. In the London syllabus, as you all know, the subject-matter is Euclid I-IV, and it is to be geometry and nothing else. "Euclid's proofs will not be insisted on, but all proofs of geometrical theorems must be geometrical." Also no questions are asked on the book-work of proportion geometry, but some years ago they put in the proviso that "the use of properties of similar figures will be allowed." When this syllabus was drawn up most of those teaching geometry had spent a great many years of their lives going through Euclid I-IV, with Book VI added, and knew what it meant. I think now-a-days it would be advisable to word that syllabus in some other way. Whether it is advisable to adopt the Cambridge syllabus is a matter for discussion.

A subject often mentioned as desirable is Numerical Trigonometry, and London does not cater for that, though it does not forbid it. I have known attempts to solve geometry riders by means of trigonometry. It really means that instead of learning something by heart about the 30°, 60°, 90° triangle, candidates use their knowledge of cos 30° and cos 60°. London does not say this must not be done. There is a great deal of variety in the Certificate examinations, as to where to put elementary trigonometry. Some put it with arithmetic and some with geometry. If it is put in with arithmetic it is generally an alternative; it is not compulsory. Alternative questions are set on trigonometry in the arithmetic paper, so that there is no *compulsion* to teach pupils trigonometry. In the Cambridge and in the Bristol School Examination trigonometry comes in with geometry, and it is not permissive. It does not say candidates must pass in trigonometry, but there it is; it is part of the syllabus, and pupils who take geometry are supposed to have done some trigonometry. My own opinion is that those who are so keen on having numerical trigonometry put into the syllabus regard it as an easy alternative either for harder arithmetic or for geometry questions. The opinion I gathered when examining for Bristol was that there were a good many candidates scraping through because they could do trigonometry, who had a poor knowledge of geometry. From that point of view some may think it an advantage to have trigonometry.

London allows schools to send up their own syllabuses and they may be examined on them. I do not know how many schools do that now—I know one; there may be more—but I find that in 1920 there were three special papers set; for one of them there were six schools; for another there were three schools; and another school had a paper all to itself because it wanted it on Friday instead of Tuesday—why, I do not know, there was no difference in the syllabus. Recollect this was in 1920. At the top of the official paper there was this note: "Candidates must not use Tables of Logarithms in working this paper." At the top of each of the special papers were the words, "Candidates may use Tables of Logarithms in working this paper." Looking through the special paper set for six schools the only difference, as far as I can see, is that three of the ordinary questions were omitted and three questions put in, involving the use of logarithms and a knowledge of indices. I am pointing this out because in those days logarithms were not allowed, but there was a strong feeling that they ought to be included in the syllabus. The fact that nine schools sent in a demand for special papers including logarithms and indices, must have brought home to the authorities that there was a strong demand for the inclusion of logarithms, and very likely this influenced them a good deal, for they have since changed the *Regulations*. So if you are keen on change, one way of bringing it to the notice of university authorities is to ask to have a special paper set and to keep on making yourself

a nuisance. Then the authorities think there is something in it and change the *Regulations*.

I should like to tell you something about the alternative syllabuses at Durham. It is stated that "Candidates must take either Scheme I or Scheme II," and I believe Scheme II would appeal to a good many people. I think the idea is to provide a rather more practical scheme for those who are going in for some of the numerous industries that are carried on in the north. Under Scheme II we read: "Use of decimals in all Arithmetic Calculations; use of rough checks for all calculations; estimate of justifiable significant figures in calculations; percentages; powers and roots; ratio and proportion with applications." I think that is intended to prevent mere puzzles being set in arithmetic papers and to keep the questions practical. Elementary Algebra—very little difference. "... variation with a simple introduction to functionality; indices with the convention for fractional and negative indices; logarithms, with the use of tables or slide rules; quantities connected by linear relations—the straight line law, its meaning and the method of determining the constants; determination of linear laws from experimental data, including cases where the law, by a simple transformation, becomes linear." * In Geometry they give a very short syllabus comprising about twelve items. "The greater part of these theorems may be taken intuitively, but deductive proofs of the following will be expected," and then follow the twelve items, after which the syllabus continues: "The elementary properties of the more common geometrical solids, their volumes and surfaces. . . . Simple methods of determining areas of irregular plane figures by the mid-ordinate rule or by counting the squares on sectional paper." † Then follows a little trigonometry. It is a rather interesting syllabus, but it is somewhat of a nuisance to set papers for it.

Mr. **C. L. Beaven** (Royal Military Academy) said he had originally been asked to open the discussion, but as a very good substitute had been provided in Mr. Paterson, he made no apology. The speaker went on to say that he had had a certain amount of experience in examining for a syllabus where trigonometry was an alternative to arithmetic, but his mind was not yet made up as to whether that was an advantage or otherwise to the candidate. It seemed that, as a rule, there was quite enough in the arithmetic alone for the average candidate. On the other hand, he was quite convinced that the better candidates got good value out of the time spent on trigonometry, though he had a feeling that some candidates attempted trigonometry when insufficiently prepared and that they suffered thereby, whereas they would not have suffered had they stuck to the arithmetic. If trigonometry was taught, how much of the school period was to be given to it? Sufficient to enable the pupils to get a decent working knowledge, or were they merely to have a smattering? Was trigonometry to be compulsory or alternative? If, as he had heard suggested, it was put on the geometry paper, then the geometry which was at present badly done would be worse. It might be well to have the three papers mixed, including questions on trigonometry in each. If there were more questions than were expected to be answered, there could be trigonometry for those who wanted it. Personally he thought it would be better to mix the papers rather than to have a special one. He thought that 40 to 50 per cent. of those who tackled questions on trigonometry showed great unfamiliarity with the subject. What was to be included in numerical trigonometry? He took it it really meant the solution of triangles rather than properties. Then how were the triangles to be solved? Would $\frac{\sin A}{a} = \text{etc.}$ be permitted? Would the cosine rule be allowed? Was that all to be done by means of the right-angled triangle? On one question he had had as many

* *University of Durham, Regulations for the Award of School Certificates*, 1928 and 1929, p. 16.
† *Ibid.* p. 17.

as twenty-four different solutions. Each case really had to be taken on its merits, but for the examiner life was not worth living! If the syllabus was to be altered at all, then those concerned should say, for the sake of the candidate, what exactly they wanted. As an examiner in trigonometry, he was not sure whether unless a fair amount of time was spent on the subject it was possible to get reasonable results. In elementary trigonometry, was it or was it not desirable that elementary solids should be included? He had had the point raised as to whether a question was legitimate which involved the visualization of elementary solids that the ordinary schoolboy or girl would know something about. For example, a question concerning a camera stand or something of that sort.

Mr. **A. W. Siddons** (Harrow) was only interested in the London School Certificate Examination to the extent that he was one of three members of the Mathematical Association who occasionally had a paper sent them with the comment, "Please curse this paper and get something done about it." There were a certain number of papers that did call for criticism, and it was difficult to know exactly how to get at the various examining bodies. The Oxford and Cambridge Joint Board examined the boys he taught and the comment he made on the papers set was that the difference between the Pass and the Credit Standard was insufficient. There should be greater distinction between the two standards. The Oxford and Cambridge Joint Board had three mixed papers, the first three questions on the paper being compulsory, one always being on arithmetic, one on algebra and one on geometry, there being choice as to the other questions; this arrangement met the case of those who feared that, when there were mixed papers, boys would get through without doing any geometry at all.

He had been teaching for thirty years and most of that time he had been battling with Examination Authorities. The changes that had taken place were interesting. The speaker said that some twenty-five years ago one examining body passed a resolution that logarithms might not be used in any examination in which the full theory of logarithms was not included in the syllabus. The full theory of logarithms! Had it yet been developed? He was not sure it had. There had been considerable advance in view of the fact that it was now possible to use logarithms in most examinations.

He was glad Mr. Beaven had put in a word for solid geometry, because for many years he had been struggling to get people to see figures in three dimensions. If there was trigonometry at all, it should include trigonometry of three dimensions. When preparing boys for the School Certificate he felt very strongly that the examination should be ignored up to a certain point, and then during the two, or possibly three, terms preceding the examination there could be a return to the syllabus itself. In his school, boys were in the Certificate Form for three terms before taking the examination, and up to that time they were taught what was best for their education, an eye always being kept on the syllabus. When the boys were put into the Certificate Form they were divided into mathematical divisions, some being tied down to the Elementary syllabus; others spending one term, possibly two, on other things; some boys would want just a term of revision work while others could ignore the examination until it came. There was a good deal to be said for such a system; certainly it was a pity to try to put in a little bit of trigonometry in the last few weeks before examination. It should be taken at least six months, if not twelve, beforehand, and when it came to the point it could be worked up again.

He would like to see both the sine and cosine rules included in trigonometry. It was, no doubt, a matter of considerable interest to see that the sine rule did apply to the right-angled triangle, but that was not fundamental. So far as his school was concerned, the use of Log sine tables had been barred up to a certain point, natural tables being used. At the next stage the boys

were not allowed to use natural tables for a few weeks; after that they were given freedom. There was considerable danger of geometry becoming dead in schools, though he did not say that was imminent. On the whole, he would rather see trigonometry put into the arithmetic paper than into the geometry paper. It seemed to be forgotten that one could judge the knowledge pupils had of Arithmetic and Algebra in a much shorter time than their knowledge of Geometry. He thought examining bodies might give more time for geometry and perhaps shorter time for other subjects. It seemed to him that the idea of introducing trigonometry into the arithmetic paper was to prevent dullness. Pupils were bored by merely grinding away at the ordinary arithmetical syllabus. It was worth while bringing in trigonometry, except in the case of a few very dull pupils, whether it was to be taken in the Certificate Examination or not.

The **Chairman** interposed the remark that if any alterations were made in the syllabuses the question as to whether they should be amended in this or that form, whether trigonometry should be put in, was one question, and the problem as to how the amount of trigonometry put in should be taught was, to a certain extent, a separate problem. He wondered, for example, whether the quality of the teaching of trigonometry in, say, the London area would not be very much improved—after a few years—if trigonometry was included, because it would lead to a body of criticism from examiners and others as to the faults that could be inferred in the teaching.

Miss **E. M. Read** (King's Norton School, Birmingham) said trigonometry was taken in her school from the bottom straight up; it was not a case of doing it during the last few months or even the last eighteen months. The school used to have a five-year course in which trigonometry was not taken until the second year, and in that year the pupils did the tangent of an angle only; in the third year they did the other five functions, and in the fourth year further practice in those functions, and certainly a good deal of work on the three-dimensional problems. In this connection the speaker agreed with Mr. Beaven and Mr. Siddons that that was the main reason for including trigonometry in a course. During the last year the pupils did a little algebraic trigonometry, the cosine rule and so on. It seemed to the speaker that if secondary pupils were to do geometry that paper must be kept separate. If there was any way of getting out of it, 90 per cent. of the pupils would do so. The Northern Joint Board did keep it separate because it put the trigonometry into the arithmetic paper. If there was no room for trigonometry in the middle school at King's Norton, then room was made for it by dropping arithmetic altogether, perhaps for a term, or for half a year, and then dropping trigonometry for the other half. The speaker thought it was possible to manage to keep all going and come out fairly at the end of the course. It was hard on pupils who had no mathematical imagination to have to spend a great deal of time on what was rather foreign to their natures. Mr. Durell had rendered great service when he wrote his *Concise Geometry*. And of course a pupil must be able to argue subconsciously in order to do even numerical work. The speaker thought that girls liked numerical trigonometry. She found that B and C forms benefited most, because they were the pupils who wanted something practical and who had not the same power of abstract thought as cleverer pupils. When the mathematics they had been doing was rather abstract algebra they had been bored, but had quite livened up when shown how to use trigonometrical tables and to ascertain how far off a ship was, or something else they thought worth while. With algebraic trigonometry which, to some extent, was included in the Northern Joint Board, one got back to the more stereotyped sort of thing. Mr. Siddons had said that the London examiners would, at one time, not have logarithms unless they could have all the theory. The Northern Joint Board asked for some theory, but the speaker thought that a mistake. She did not mean that pupils should

be taught without explanation, but thought that schoolgirls could manage to understand without being able to tell why such and such a thing was the case, and therein lay the difficulty of examining: it was impossible to tell whether children had been taught properly or not, if they could not tell what they knew. The speaker did not think it was true that in order to teach trigonometry it was necessary to teach the theory of similar triangles. All that was necessary was subconscious knowledge, such as one had as soon as one knew that a picture in a book represented daddy. Trigonometry was taught in her school for a couple of years before the pupils knew anything about the theory of similar triangles.

Mr. **A. Montagnon** (Leeds; Secretary of the Yorkshire Branch) said that those who taught in the great industrial areas of the north of England regarded numerical trigonometry as such an important subject that it should be definitely included in a Lower Mathematics scheme. It was included in the arithmetic paper of the Northern Board, and it should be borne in mind that that Board now divided their mathematics papers into two sections, A and B, an innovation which was made two or three years after the war, and which arose, incidentally, out of a stampede by several headmasters over one algebra paper. The Board had more or less agreed that the object of Section A was to examine a candidate as regards mere knowledge and accurate working. In fact in the new *Regulations* it was stated in heavy type that the candidate need not show more working than he considered sufficient to get the answer. As regards Section B it was also stated in heavy type that as to the four harder questions, detailed explanations must be given of the arguments involved. The alternative trigonometrical questions were in Section B, and the syllabus said: "Simple numerical Trigonometry; trigonometrical functions of one angle"—that included obtuse angles—"problems involving the solutions of triangles"—obtuse angles naturally included—"height and distance problems and the use of Log tables will be required." It might be interesting to those present to know the type of questions to which the trigonometrical were alternatives. In the arithmetic set in 1929 the first question in Section B was a mensuration question on the cylinder divided into two parts; the second was a graph question on compound interest; the third a question on stocks and shares; and the fourth one on proportions and averages. The trigonometry alternatives were, firstly, a calculation question, which would go along with the mensuration question; the second question was definitely one in three dimensions though, admittedly, it could be solved in two. The realization of what the problem demanded was in three dimensions. In the third question the pupils were asked to find the length of a median and calculate an angle. The fourth involved calculating a side of a triangle and also its height.

The speaker in detailing the advantages of the Northern Board syllabus said it helped considerably in any geometrical work which involved similar triangles, and tended to bring continually before the pupil's eyes on other occasions than geometry lessons the significance and the importance of similar triangles, and he supposed he might add to that the fact that similar triangles also played a part in helping forward ordinary geometry.

Then, he thought it might be said that the syllabus extended the graphical work by including the possibility of drawing the sine and cosine curve, the tan curve and also the log curve, and incidentally the log curve was asked one year in one of the various papers of the Northern Board. It also tended to complete the mensuration questions called for in arithmetical syllabuses by including the possibility of the giving of angles.

There was an important side line which had been omitted altogether, and that had surprised him more than anything else, namely, that there had been no reference during the discussion to Applied Mathematics. It seemed to him and to others in the north that it was very important that in whatever

part of the examination Applied Mathematics might come, even if only in the physics, the pupil should stand some hope of being familiar with the sine and the cosine. That made the march towards the ultimate goal of an understanding of the principles of Applied Mathematics very much more rapid, and allowed of doing what he thought he might call accurate work in mechanics as against mere practical work. In other words, one could intertwine the two and find examples of trigonometry from the mechanics and assist mechanics by knowledge of trigonometry.

In its graphical work he thought the syllabus had a distinct advantage from the point of view of what was to happen afterwards. It was all very well to discuss the School Certificate, but it should not be considered apart from the ultimate object. If the pupils after taking the School Certificate were to go into the VIth Form and proceed, surely the graphical work involved was of great use in introducing those pupils to functionality.

He had been interested in Mr. Beaven's remarks as to what he had found when examining the Northern candidates, but there were two aspects to that question: the aspect seen by teachers and the aspect seen by the examiner. He said that more freely because before he went to the north of England he had helped to examine for the Northern Board. Before he went north he had felt there were many things which were not being done properly, but when he saw the pleasure given to the lower sides of schools, to the really down-and-outs, because they felt that at last they had something tangible, something which belonged to the realm of their own living and not just merely to a class-room, he was quite prepared to forgive them Mr. Beaven's 40 to 50 per cent. If they could get that, it was something. There was no doubt that numerical trigonometry could be taught just so far as a teacher felt his pupils were able to go.

As regards time, the speaker was convinced—and he thought others from the north would agree with him—that it was possible, in one period a week throughout a whole year, to teach sufficient numerical trigonometry to deal, at any rate, with what the Northern Board wanted. He did not say that teachers had then taught their pupils trigonometry. That was a very different matter. It was, however, highly desirable to do what teaching was possible in that time, and that was where those in the north mostly differed—as to whether logs should or should not be studied beforehand. Most schools which had tried to run trigonometry successfully also ran their logarithms in the year preceding; they made no attempt to teach the proofs of the log theorems, but simply the uses. In his opinion it was necessary to do the practical side of log work, so to speak, the year before, and thus give the pupil the means of linking up all log tables rapidly and quickly. He thought numerical trigonometry was a means, not only of continuing the work done before and of solidifying it, but also of preparing the ground for work that was to come —a most important factor.

In Geometry he thought that the Northern Board had done something very sensible when they put down in black and white exactly what was required; there was a complete list of theorems that were expected to be proved or not, as the case might be, and a complete list as to what was to be done on the practical side. There was a distinct advantage in having a really definite and tangible scheme laid down by a Board.

Mr. **A. L. Atkin** (L.C.C. Inspector) said that he would not have risen had not two points very dear to him been mentioned, namely, similarity and solid geometry. He regarded even an elementary mathematical education as of little value if solid geometry was omitted, and held that it should appear comparatively early in the training of anybody who had the prospect of becoming a mathematician. In solid geometry he saw the salvation of what had been spoken of, somewhat despairingly, namely, deductive geometry. He was convinced that the reason why many were so disappointed with their

efforts in the teaching of deductive geometry was that they were, in fact, attempting to train children in the application of a method, dealing with material with which the pupils were not familiar. It was because of that that the reformers of the teaching of geometry suggested a course preliminary to formal deductive geometry, and the difficulty had since been to find what was suitable material for that preliminary course. He thought it undesirable that there should be any attempt to undermine the deductive treatment of geometry later on by hauling out from Euclid the more striking of his theorems and leading boys and girls to become acquainted with them by methods which were not deductive. When this was done a good deal of the real interest of the study of formal deductive geometry was gone so far as pupils were concerned. The introduction of similar triangles at a very early stage and before the pupils knew anything about formal deductive geometry did certainly add to the scope of the work; and, if carried out in the way that Miss Read had suggested, would render pupils at a later stage in their school career so thoroughly familiar with the subject that the crimes of which Mr. Beaven had spoken would not be so common. Exercises in solid geometry could not only very profitably be included before pupils took formal deductive geometry, but it was possible for formal deductive geometry from the outset to be very much enlivened thereby. He thought all would agree that the starting point of formal deductive geometry must necessarily be the application of the congruence of triangles to riders, but many of the exercises were singularly dull. There was no harm in enlivening them by taking, for example, a cube in perspective and asking pupils to take certain triangles and show them to be congruent.

The **Chairman** believed that quite a number present found themselves in the same position as he frequently found himself. How many who took the London Examination taught trigonometry for two or three terms or more, but could not present it because that could only be done through the Advanced paper ?

About twenty of those present having signified that they found themselves in that position, the Chairman, continuing, thought he was voicing the opinion of those members when he said that they would like to have an opportunity, he would not say of showing off their children in trigonometry, but of being relieved from the position of being compelled to drop trigonometry, as most were compelled to do, even in the third year. If one had a weak class which was to be examined it was quite enough for them to do to concentrate on the actual syllabus. As a matter of fact, he, personally, did not entirely drop trigonometry in those stages because he used the simple properties which helped in geometry. That applied not to A Form but to B.

Mr. **G. L. Parsons** (Merchant Taylors' School) commended the practice of the Oxford and Cambridge Joint Board, which he thought nearer perfection than many other practices, namely, that of having three mixed papers, which now consisted of nine questions. Three of the questions in each paper were alleged to be compulsory; in practice, he thought the compulsory questions were generally done because they were easier than others. Out of the remaining six questions a candidate must do four, in order, that was to say, to get 100 per cent. though he could very well get his credit with a good deal less. In general, those six questions were split up, two to each subject, but with the reservation that the line of demarcation was by no means rigid. There might be a question involving a certain amount of geometry mixed up with a certain amount of algebra, or a lot of arithmetical calculation on a geometrical figure and so on. Personally, he thought that a much more liberal way of examining candidates than tying them down to nose along one particular line. It had been suggested by one speaker that that led to the neglect of geometry. As a matter of fact, he did not believe that was so in principle. What one found was that it led to certain candidates rushing at

the geometry questions in the hope that they would score easy marks. The candidate fondly imagined that out of, say, 16 marks allotted to the total question he would get about 12 for the theorem and about 4 for the rider when, in actual practice, the proportion was reversed. Thus it led, not to neglect of geometry, but to a good many candidates diving into geometrical questions which were beyond their capabilities. There was, he thought, in the procedure he had outlined a solution which would meet the objections of members from the north, for if there was a wish that there should be included in the Elementary Mathematics syllabus other subjects than those already included, if there was a system of alternative questions, there was no earthly reason why such inclusion should not be possible.

There was just one point in Mr. Siddons' speech that had rather surprised him, namely, the disparity between Pass and Credit standard. From what he had seen, he had always thought that the Pass standard was rather easy in the examination of the Oxford and Cambridge Joint Board.

Mr. **F. G. Firth** (St. Olave's) regarded the geometry syllabus for the General School Certificate as very unsatisfactory. Theorems were set in examination which did not appear in the book. It would be a great advantage if it could be stated what theorems were expected and what not.

Miss **R. H. King** (Colchester Girls' High School) said that in the geometry paper for the Cambridge School Certificate there were definitely a certain number of questions on elementary trigonometry. Both the Cambridge School Certificate and the Oxford School Certificate and the Oxford and Cambridge Joint Board had the same procedure as the Northern Universities Joint Board of giving detailed syllabuses in the geometry and marking certain theorems of which proofs would not be expected. The speaker emphasized the importance of a statement that proofs would not be expected of certain fundamental theorems. In regard to the examinations she had mentioned it was said quite definitely that proofs of the properties of parallel lines would not be expected, and in most cases that proofs of congruent triangles would not be expected, and also that the theorems were not expected to be proved for equal arcs in the same or equal circles subtending equal angles at the centre. Comparison of the four syllabuses showed that in time given to the papers, as in other ways, London came off very much the worst.

Mr. **S. Inman** (Isleworth County School) thought that some resolution embodying the feeling of the meeting should be forwarded to the London University. There was no doubt that most schools were practically forced to do what the syllabus suggested. If there was no numerical trigonometry in a syllabus, a very large number of schools either did not treat it at all or treated it insufficiently. Which was the best from the educational point of view: to force a pupil to learn up 60 theorems which he would soon forget, or to teach him such subjects as trigonometry? Again, he did not think mensuration was treated at all sufficiently, and yet it was really a very important subject. He thought that the committee should get together to frame some resolution, and suggested that a recommendation be sent up that numerical trigonometry be included in the syllabus; that further details be given; for instance, instead of "Euclid I-IV," it was time the London syllabus gave definite details. He did not know why other syllabuses were definite and London syllabuses nearly always vague. He did not see why examiners should be afraid to state what they expected to be known.

The **Chairman** took it that the purport of the resolution was as follows:

"That the Committee of the London Branch be authorised by this meeting to approach the Examining Body of London University with a view to effecting certain important alterations in the existing Syllabuses, especially in the direction of the inclusion of Numerical Trigonometry and the modification of the Geometry Syllabus."

It would strengthen the hands of the Committee of the London Branch if, in making some such series of proposals, it had behind it the backing of that meeting.

Mr. **F. C. Boon** (Dulwich) seconded the resolution, and added that examinations should not be regarded as something which took those in schools out of their stride, but rather as something which could be worked steadily towards without more than a casual eye on the syllabus. Those twenty members or so, of whom he was one, would no doubt feel as strongly as he did that it was a very regrettable thing that pupils of sixteen having made a fair start on trigonometry were compelled, for perhaps the last year they did mathematics, to give it up owing to urgency of examination. Those in charge might feel sure that a boy would pass comfortably up to the Credit standard in ordinary elementary mathematics, but they would not risk the failure of the average boy by compelling him to go on with trigonometry when the boy himself felt he could not do the two. He did not know whether a further suggestion might be made, namely, that the London University should do what the Cambridge Joint Board had done, namely, have a paper in which more questions were set than need be answered, and that both London and the Oxford and Cambridge Joint Board should include in the second part of the paper some trigonometrical questions. That was not in order that there might be a soft option, but that pupils who had got a certain way with their trigonometry might continue the subject with the certainty that they would have the chance of using it to get their credit in the examination.

The **Chairman** then put the resolution, but said that the precise form in which it would be sent up would have to be hammered out by the Committee of the London Branch.

The resolution was approved, there being only two dissentients.

A vote of thanks to Mr. Paterson and to others who had taken part in the discussion terminated the proceedings.

The meeting, at which the attendance was 100, was the last of a session in which the average attendance had been 68.

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769. The very learned Dr. Peacock, writing on the history of arithmetic, speaks of Stevinus and Simon of Bruges as two different persons. . . .—*Athenaeum*, 1859, ii.

770. We see by an examination paper recently set at St. John's College, Cambridge, that arithmetic is slowly making its way. . . . The possibility of a fraction having its terms *concrete*, at which the whole University was frightened a few years ago, is again recognised. . . . We find [the following question] in the paper above mentioned :

"Any attenuation of an homoeopathic medicine is made by taking one part of the previous attenuation with 99 of a non-medicinal substance : the globules weigh $\frac{1}{4}$ grain each. A person taking 6 grains a day for 4 weeks recovers from illness : how long ought he be recovering if he took 6 globules of the 12th attenuation daily ? If he actually recovers in 6 weeks, compare the efficacy of his imagination with that of the medicine."

According to the examiner's theory a bushel of medicine would have certainly wrought a cure in next to no time ; and certainly the patient would soon have ceased to feel—unwell.—*Athenaeum*, 1859, ii. p. 891.

771. Long ago, two computers, in two different parts of England, computing in duplicate for the "Nautical Almanac," and perfectly ignorant of each other's existence, made one wrong figure exactly in the same way in one place, in a particular lunar distance. Their answers agreed to a nicety ; and it was only when a staring error was pointed out to the superintendent in the printed book, that an examination of the original computations detected this almost incredible coincidence. But it never happened twice.—[? De Morgan] *Athenaeum*, 1858, i. p. 334.

DECIMAL PROCESSES: "TRACKING THE UNIT."

By F. C. Boon, B.A.

Twenty years ago it was anticipated that "Standard Form" would prove the ideal method for decimal processes. It was designed to abolish the mechanical teaching of a mechanical rule and to base arithmetical instruction on a conscious grasp of principles. A wide and extended trial has not realised the anticipation, indeed the method appears to-day to have many followers but few champions.

Mr. Kearney's paper in the May *Gazette* may be taken as a symptom that teachers are still searching for a method which, while having the advantage of mechanical simplicity, will contribute to arithmetical progress by insistence on the simple and direct application of a fundamental principle.

Such a method must be mechanically smooth in operation and free from pitfalls; it should depend on and be a reminder of a fundamental principle. It is desirable that the principle may be such as can be expressed in a simple formula, which can be widely applied in other parts of the subject.

I believe that the method outlined here, known as "Tracking the unit method," has these and other advantages. Those who have given it a long trial prefer it to other methods. But it is little known. It deserves at any rate a wider trial.

Whatever rule is used for multiplication and division of decimals must depend ultimately on the abacus and the symbolic representation of it that we use under the name of the Arabic system of numeration.

It is implicit in the system that the units position is the "origin" of place-value. The development of the system depends on the principle "To multiply by 10, move the figures each one place to the left." This leads to the rules for multiplication and division by other powers of 10; but, as a mnemonic, the above formula suffices and is regularly referred to. The principle with its variations appears in concrete form on the abacus, the use of which should provide a permanent visual association.

The formula will be recalled at various stages of instruction.

Find the dividend on £390 12*s.* 0*d.* *at* 18*s.* 3*d.* *in* £1.

$$\begin{array}{r|l|r} & & \text{£}390{\cdot}6 \\ \hline 18s. & \text{£}\frac{9}{10} & 351{\cdot}54 \\ 3d. & \text{£}\frac{1}{80} & 4{\cdot}8825 \\ \hline & & \text{£}356{\cdot}4225 \end{array}$$

In class work this sort of catechism proceeds:

How do you find $\frac{1}{10}$? Move one place to the right.
How do you find $\frac{9}{10}$? Multiply by 9 and move one place to the right.
And to find $\frac{1}{80}$? Divide by 8 and move one place to the right.

Find 3% *of* £931 8*s.* 6*d.*

$$\frac{3}{100} \text{ of } \text{£}931{\cdot}425$$
$$\text{£}27{\cdot}94275$$

Catechism: How do you find $\frac{1}{10}$? Move one place to the right.
How do you find $\frac{1}{100}$? Move two places to the right.
How do you find $\frac{3}{100}$? Multiply by 3 and move two places to the right.

By selling an article at £1 9*s.* 7*d.* *a man lost* 10%. *What was the cost price*?
The cost price was $\frac{10}{9}$ of £1·47916. *i.e.* £1·6435.

Catechism: How do you find $\frac{1}{9}$? Divide by 9.
How do you find $\frac{10}{9}$? Divide by 9 and move one place to the left.

In this application of the principle, the decimal point, which is in its nature a mark of position, does not move. The figures move, changing their denomination (units, tens, hundreds, etc.) according to the denomination of the operating digit.

In practice the attention is directed to the unit's figure rather than to the decimal point. The denomination of a digit is determined by its displacement to left or right from the unit's digit. This gives, later on, the simple rule for the characteristic of a logarithm. "The characteristic is the number of places that the leading digit is removed from the unit's figure; it is positive if the displacement is to the left, negative if to the right."

It will be seen that the preparation for the application of this principle to the multiplication of decimals begins at a very early stage.

In

$$\begin{array}{r} 63 \times 10 \\ \hline 63\cdot \\ \hline \end{array}$$

the digits being moved one place to the left, there is nothing to mark the unit's place. A zero is inserted.

[The zero (Ar. *alsifr* = emptiness) was invented to mark a place in which there was no digit, i.e. to indicate a blank in the abacus reckoning.]

In

$$\begin{array}{l} \;\;63 \times 12 \\ \hline 63 \\ 126 \\ \hline 756 \\ \hline \end{array}$$

the unit's place is marked by a digit in another partial product and no 0 is necessary.

In

$$\begin{array}{r} 837 \div 10 \\ \hline 83|7 \end{array}$$

the digits being moved to the right for division by 10, the 7 occupies a position, which in the arithmetic of integers has no denomination.

To fix the unit's place the decimal point is introduced, and a suitable denomination introduced for the extension of place value thus developed.

Multiplication of decimals.

$327{\cdot}41 \times 130{\cdot}7$

$$\begin{array}{r} 327{\cdot}41\;\; \\ 130{\cdot}7\;\;\; \\ \hline 32741\;\;\;\;\;\;\; \\ 9822{\cdot}3\;\;\; \\ 229{\cdot}187 \\ \hline 42792487 \\ \hline \end{array}$$

The catechism proceeds:

What does 1 represent? 100.
How do you multiply by 100? Move two places to the left, and so on.

And the mechanical rule becomes:

Move the partial product as many places to left (or right) as the multiplying digit stands to left (or right) of the unit's position.

It is seen that in this method there is no rearrangement of the form of the question and that every digit written down has its correct denomination (or place value).

In such a case as $623{\cdot}7 \times 0{\cdot}071$

$$\begin{array}{r} 623{\cdot}7 \\ 0{\cdot}071 \\ \hline 43{\cdot}659 \\ {\cdot}6237 \\ \hline 44{\cdot}2827 \\ \hline \end{array}$$

it is essential that the 0 in the unit's place be inserted even if it is not given in the question.

[*N.B.*—The rule of adding the numbers of decimal places can be used as a check in these examples.]

It will be noticed—and occasionally the pupil should be made to notice—that in each partial product the digit derived from the unit's figure of the multiplicand takes the denomination of the multiplying digit. These digits are marked in the last two examples in heavy type. The pupil will prefer to see and to say this in this way:

The digit derived from the unit's digit of the multiplicand is directly below the multiplying digit.

This could have been shown earlier in long multiplication of integers.

Division.

$9379 \div 371$

$$\begin{array}{r} 25 \\ 371\ )\overline{9379} \\ 742 \\ \hline 1959 \\ 1855 \\ \hline 104 \end{array}$$

Here we have the converse of what was noticed in multiplication. The digit shown in heavy type is derived from the unit's digit of the divisor, the corresponding digit of the quotient is directly above it. Thus as the unit's digit of 371 has taken a ten's denomination in the first subtrahend, the multiplying digit was a ten's digit.

This rule is applied literally to division of decimals.

$9379{\cdot}62 \div 371{\cdot}03$

$$\begin{array}{r} 25 \\ 371{\cdot}03\ )\overline{9379{\cdot}62} \\ 7420{\cdot}6 \\ \hline 1959{\cdot}02 \\ 1855{\cdot}15 \\ \hline 103{\cdot}87 \end{array}$$

The one difficulty in the whole application of the "Tracking the unit method" is in the first step of division. The first subtrahend must be written down before the digit of the quotient is written. But it is a small difficulty.

There are a number of checks on the correct placing of the quotient. At the stage at which the above example stops, viz. when the unit's figure of the quotient has been reached, there are two things to notice:

(1) The remainder is for the first time less than the divisor;

(2) The number of decimal places in use at the moment is the number of places in the divisor.

In such a case as $89{\cdot}3 \div 0{\cdot}037$ the rule is still applied literally, but the unit's digit 0 must be used in the first subtrahend, and is unnecessary after.

In this example it is used in each subtrahend to emphasize the point that

each digit derived from the unit's 0 of 0·037 is directly below the corresponding digit in the quotient.

$$
\begin{array}{r@{\,}r}
 & 2413 \\
\cline{2-2}
0{\cdot}037\,) & 89{\cdot}3 \\
 & 0074\phantom{{\cdot}0} \\
\cline{2-2}
 & 15{\cdot}3 \\
 & 014{\cdot}8 \\
\cline{2-2}
 & {\cdot}50 \\
 & 00{\cdot}37 \\
\cline{2-2}
 & {\cdot}130 \\
 & 0{\cdot}111 \\
\cline{2-2}
 & {\cdot}019
\end{array}
$$

[The checks for division mentioned above may again be noticed.]

In such a case as $903{\cdot}5496 \div 29{\cdot}143$,

$$
\begin{array}{r@{\,}l}
 & 31{\cdot}004 \\
\cline{2-2}
29{\cdot}143\,) & 903{\cdot}5496 \\
 & 874{\cdot}29 \\
\cline{2-2}
 & 29{\cdot}259 \\
 & 29{\cdot}143 \\
\cline{2-2}
 & {\cdot}116600 \\
 & {\cdot}116572
\end{array}
$$

the tracking of the unit's figure in the last subtrahend checks the position of the 4 in the quotient and assures the none-too-confident pupil that his two 0's are correct.

It is especially an advantage in division that each digit has its correct denomination, as the remainder is in this method a correct remainder.

As a conclusion to what is necessarily only a brief outline of a method, it should be said quite emphatically that the method brings no difficulty with it when contracted methods are to be learned and is applicable directly in questions where any number of successive operations of multiplication and division are involved.

F. C. BOON.

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772. The decipherer of Egyptian hieroglyphics, Dr. Thomas Young . . . was acquainted with a score of languages, and, moreover, he could ride two horses at a time, dance on the tight rope, and play harlequin.—*Athenaeum*, 1858, i. p. 183.

773. There are those who like to know the precise time and manner of all things: let them stand informed that the official recognition of the continental school of mathematicians at Cambridge dates from nine o'clock in the morning of Monday, Jan. 13, 1817, when Peacock put into the hands of each candidate for honours a printed paper, the fourth question of which stands thus:

"Find the integral of $\dfrac{dx}{1+x^3}$."—*Athenaeum*, 1858, ii. p. 649.

774. When Lalande's large work on Astronomy was published, a contemporary called it *La Grosse Gazette*, alluding to the quantity of miscellaneous gossip therein contained.—*Athenaeum*, 1858, ii. p. 793.

775. We cannot undertake to describe in full what [Peacock] did for Algebra. . . . If his opinions do not find active and successful supporters, in twenty years Oxford will be the great school of the exact *disciplines* in England, and Cambridge will be but the Epsom or the Doncaster of bookwork and problem races.—*Athenaeum*, 1858, ii. p. 650.

PROBLEM BUREAU.*

THE following problems recently sent to the Bureau seem of sufficient interest to justify insertion in the *Gazette.*

1. *A bracelet is composed of similar beads of two different sizes, three groups of small ones, each $2n$ in number, being separated by three single larger ones. Show that the bracelet may be restrung in $3n(n+1)$ distinct ways* (Univ. of Edinburgh; examination for the Diploma in Actuarial Maths. 1926).

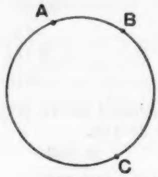


FIG. 1.

Suppose the $6n$ small beads are placed on the bracelet, and consider how the 3 large ones can be placed between them.

Let A and B be the two large ones closest together; then there can be 0 or 1 or 2 ... or $2n$ small ones between A and B, and there must be more than that number between C and both A and B.

If there are :

0 small ones between A and B, then there are					$3n+1$	places in which C can be.			
1	,,	,,	,,	,,	$3n-1$	,,	,,	,,	
2	,,	,,	,,	,,	$3n-2$	,,	,,	,,	
3	,,	,,	,,	,,	$3n-4$	,,	,,	,,	
....									
$2n-1$	,,	,,	,,	,,	2	,,	,,	,,	
$2n$	,,	,,	,,	,,	1	,,	,,	,,	

and the last gives the original arrangement.

$\therefore$ Total number of rearrangements is

$$2+4+5+7+8+\ldots+(3n-4)+(3n-2)+(3n-1)+(3n+1)$$
$$=\{2+3+4+5+\ldots+(3n+1)\}-(3+6+9+\ldots+3n)$$
$$=\tfrac{1}{2}(3n+1)(3n+2)-1-\tfrac{3}{2}n(n+1)$$
$$=\tfrac{1}{2}[9n^2+9n+2-3n^2-3n]-1=3n^2+3n=3n(n+1),$$

and all these ways are distinct.

A. S. G. T.
J. W. H.

2. *ABC is a right angle.* $AB=a$.

A pig P starts from B and runs along BC.

At the same time a farmer F starts from A and runs n times as fast as the pig, heading always directly towards the pig. How far from B does the farmer catch the pig? (communicated by C. Kendal, St. John's Coll., Cambridge).

Let $BP=Z,\quad AF=s=nZ,\quad FP=Q,\quad \angle FPB=\psi.$

Rate of increase of FP

$$=\text{Component along } FP \text{ of pig's velocity } \frac{dz}{dt}-\text{farmer's velocity } \frac{ds}{dt}.$$

$$\therefore\ \frac{dQ}{dt}=\frac{1}{n}\cdot\frac{ds}{dt}\cos\psi-\frac{ds}{dt}.$$

* *Hon. Sec.*, A. S. Gosset Tanner, M.A., Derby School, Derby.

Multiply each by $\frac{dt}{ds}$, and substitute $\frac{dx}{ds}$ for $\cos\psi$.

$$\therefore \frac{dQ}{ds}=\frac{1}{n}\frac{dx}{ds}-1.$$

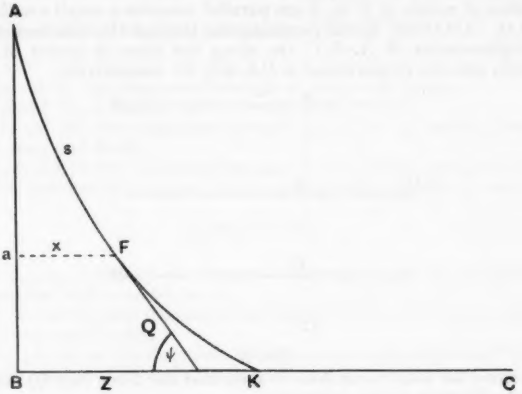


FIG. 2.

$\therefore\ Q=\frac{x}{n}-s+$a constant of integration which $=a$ from initial conditions.

$$\therefore\ Q=\frac{x}{n}-s+a. \quad [1]$$

$$\text{At } K,\ x=Z=\frac{s}{n}. \quad [2]$$

$$\text{At } K,\ Q=0. \quad \therefore\ a=s-\frac{x}{n}. \quad \text{(from [1])}$$

$$=nx-\frac{x}{n}. \quad \text{(from [2])}$$

$$\therefore\ x=\frac{an}{n^2-1}.$$

$\therefore$ The farmer catches the pig when it has run $\frac{n}{n^2-1}$ of the distance originally separating them. W. H. J.

[Compare Tait and Steele, *Dynamics of a Particle* (1st ed., 1856), p. 14. —*Editor.*]

3. *From the properties of the instantaneous centre of a lamina moving in its plane deduce the theorem that three forces in equilibrium must be concurrent unless they are parallel, and, if they are parallel, deduce the relation between their magnitudes and the distances between their lines of action* (Army Exam., June 1925, Higher Maths. Paper 2).

Let the three forces be P, Q, R acting through A, B, C respectively.

If the (rigid) triangle ABC be in equilibrium, the work done in any small displacement is zero.

Let A move perpendicular to the direction of P, and B move perpendicular to the direction of Q.

The instantaneous centre of rotation is at O, the point of intersection of the lines of action of P and Q. Also C moves perpendicular to OC.

Now P and Q do no work, therefore R does no work, therefore the line of action of R must be perpendicular to the direction of movement of C, i.e. must lie along CO.

Hence the lines of action of P, Q, R must meet in a point; if this point be infinitely distant they will be parallel.

If the lines of action of P, Q, R are parallel, consider a small rotation about any point O. Let $OABC$ be the perpendicular through O to the lines of action.

The displacements of A, B, C are along the lines of action of P, Q, R respectively, and are proportional to OA, OB, OC respectively.

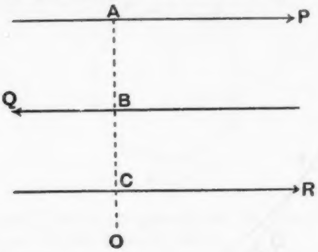


FIG. 3.

Hence, since the total work done is zero, and one force (say Q) must be in the opposite direction to the other two,

$$P \, . \, OA - Q \, . \, OB + R \, . \, OC = 0,$$

also

$$P - Q + R = 0,$$

whence

$$P \, . \, AC = Q \, . \, BC$$

and two similar relations.

E. H. N.
A. S. G. T.

4. *All the diagonals of a regular polygon of n sides are drawn: required to find the total number of triangles that are thus formed within the polygon* (first proposed by the late H. E. Dudeney for the case where $n=5$).

The triangles may be divided into four classes:

Class A.—3 vertices on the perimeter of the polygon.
Class B.—2 vertices „ „ „ „
Class C.—1 vertex „ „ „ „
Class D.—no vertex „ „ „ „

Class A.—The number is clearly ${}^nC_3 = \dfrac{n(n-1)(n-2)}{6}$.

Class B.—First to find the total number of intersections of the diagonals: Let the polygon be $A_1A_2A_3 \ldots A_n$.

A_1A_3 is cut by the $(n-3)$ diagonals through A_2 in $1 \, . \, (n-3)$ points.

A_1A_4 is cut by the $(n-4)$ diagonals through A_2, A_3 in $2 \, . \, (n-4)$ points.

A_1A_5 is cut by the $(n-5)$ diagonals through A_2, A_3, A_4 in $3 \, . \, (n-5)$ points, etc.

Total number of intersections on the diagonals through A_1

$$= 1 \, . \, (n-3) + 2 \, . \, (n-4) + 3(n-5) + \ldots (n-3) \, . \, 1,$$

$$= \sum_{r=1}^{n-3} [(n-3)r - r(r-1)],$$

$$= \frac{(n-3)(n-3)(n-2)}{2} - \frac{(n-2)(n-3)(n-4)}{3} = \frac{(n-1)(n-2)(n-3)}{6}.$$

$\therefore$ Total number of intersections on all the diagonals of the polygon

$$=\frac{(n-1)(n-2)(n-3)}{6}\times\frac{n}{4}=\frac{n(n-1)(n-2)(n-3)}{24}.$$

But for each intersection, there are four triangles of this class.

$\therefore$ Total number of triangles of class $B=\dfrac{n(n-1)(n-2)(n-3)}{6}$.

Class C. :—

A_nA_2 is cut by the diagonals through A_1 in $(n-3)$ points.

These give ${}^{n-3}C_2$ triangles with a vertex at A_1.

A_nA_3 is cut by the diagonals through A_1 in $(n-4)$ points, giving ${}^{n-4}C_2$ triangles, etc., and finally,

A_nA_{n-3} is cut by the diagonals through A_1 in 2 points, giving ${}^2C_2$ triangles.

$\therefore$ Total number of triangles, bases on diagonals through A_n, vertices at A,

$$=\sum_{2}^{n-3}({}^rC_2)={}^{n-2}C_3.$$

Again $A_{n-1}A_2$ is cut by the diagonals through A_1 in $(n-4)$ points giving ${}^{n-4}C_2$ triangles with a vertex at A_1.

$A_{n-1}A_3$ is cut by the diagonals through A_1 in $(n-5)$ points giving ${}^{n-5}C_2$ triangles, etc., and finally,

$A_{n-1}A_{n-4}$ is cut by the diagonals through A_1 in 2 points giving ${}^2C_2$ triangles.

$\therefore$ Total number of triangles, bases on diagonals through A_{n-1}, vertices at A_1,

$$=\sum_{2}^{n-4}({}^rC_2)={}^{n-3}C_3, \text{ etc.}$$

Total number of triangles, bases on diagonals through A_{n-2}, vertices at A_1,

$$={}^{n-4}C_3.$$

One triangle has its base on A_5A_2 and vertex at A_1.

$\therefore$ Total number of triangles, vertex at A_1,

$$=\sum_{3}^{n-2}({}^rC_3)={}^{n-1}C_4.$$

Hence total number of triangles of Class C

$$=n\,{}^{n-1}C_4$$

$$=\frac{n(n-1)(n-2)(n-3)(n-4)}{24}.$$

Class D :—

Any six vertices of the polygon can be joined in one, and only one way to form a triangle of this class.

The total number of triangles in Class D

$$={}^nC_6$$

$$=\frac{n(n-1)(n-2)(n-3)(n-4)(n-5)}{720}.$$

When *n is even* many of the triangles in Class D vanish owing to concurrencies among the diagonals. The determination of the formula for these concurrencies is by no means a simple matter and is especially complicated when $n\equiv M(6)$. This will be dealt with in a future number.

In the meantime the Secretary of the Bureau will be glad to hear from any member who cares to investigate the case where $n\equiv M(6)$.

Derby School. A. S. G. T.
S. J. T.

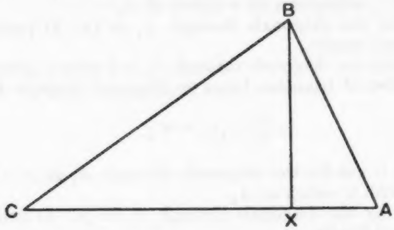
MATHEMATICAL NOTES.

961. [V. 1. a. θ.] *Solution of Triangles in Numerical Trigonometry.*

The Syllabus of Numerical Trigonometry usually excludes treatment of compound-angles and half-angles.

The solutions of Δs with the data (i) a, b, C, (ii) a, b, c, are possible by the cosine formula, but the computation is forbidding.

For (i), the following method, which is Bryan's method in symbolical form, is simple and self-checking.



ABC is a triangle: BX is perpendicular to AC.

$$\cot A = AX/BX = (AC - CX)/BX = AC/BX - CX/BX$$
$$= b/(a \sin C) - \cot C = (b/a) \operatorname{cosec} C - \cot C.$$

Similarly $\qquad \cot B = (a/b) \operatorname{cosec} C - \cot C.$

Given $a = 37{\cdot}23$, $b = 43{\cdot}71$, $C = 50° \, 29'$.

$$\cot A = {}_{10}[\cdot 1824] - \cdot 8248$$
$$= 1{\cdot}522 - \cdot 825$$
$$= \cdot 697$$
$$\cot B = {}_{10}[\cdot 0430] - \cdot 8248$$
$$= 1{\cdot}104 - \cdot 825$$
$$= \cdot 279$$

	Numerical Values.	logs.
a	37·23	1·5709
b	43·71	1·6406
a/b		$\bar{1}{\cdot}9303$
b/a		·0697
cosec C	cosec 50° 29′	·1127
cot C	·8248	

Whence $A = 55° \, 7'$ and $B = 74° \, 24'$.

(As these values depend on 3 figure results they are not reliable to the nearest minute.)

Check, $A + B + C = 180° \, 0'$.

Check in the working of :—$\log b/a + \log a/b = 0$.

Note.—$1/\sin C$ can be used for cosec C; but it may be that at the stage where this formula becomes useful, it is convenient to have a reason for introducing the cosecant.

For (ii), it is necessary to prove $\Delta = \sqrt{s(s-a)(s-b)(s-c)}$, a generalisation of what is done in Bryan's method.

From this $\qquad \sin A = 2\Delta/(bc)$, etc.

The advantage of this method is that it requires very little more working to get A, B and C than to get A, and it is self-checking.

Given $a=362{\cdot}7$, $b=413{\cdot}9$, $c=518{\cdot}2$.

$2s$ is the sum of a, b, c; the values of $s-a$, $s-b$, $s-c$ check the addition and subtraction done, because their sum $=s$. Δ^2 is obtained by adding the logs of s, $(s-a)$, $(s-b)$ and $(s-c)$: Δ, by halving the log of Δ^2, and 2Δ, by adding $\cdot 3010$ to log Δ.

	Numerical Values.	logs.
a	362·7	2·5595
b	413·9	2·6169
c	518·2	2·7145
$2s$	1294·8	
s	647·4	2·8112
$s-a$	284·7	2·4544
$s-b$	233·5	2·3683
$s-c$	129·2	2·1113
Δ^2		9·7452
Δ		4·8726
2Δ		5·1736

$$\sin A=\frac{2\Delta}{bc}={}_{10}[5{\cdot}1736-5{\cdot}3314]={}_{10}[\bar{1}{\cdot}8422].$$

$$\sin B=\frac{2\Delta}{ca}={}_{10}[5{\cdot}1736-5{\cdot}2740]={}_{10}(\bar{1}.8996].$$

$$\sin C=\frac{2\Delta}{ab}={}_{10}[5{\cdot}1736-5{\cdot}1764]={}_{10}[\bar{1}.9972].$$

Whence $A=44°\ 3'$, $B=52°\ 31'$, $C=83°\ 30'$.

Note 1.—We have to consider whether the largest angle is an acute angle or its supplement; here C is acute.

Note 2.—As angles approach 90°, the differences in the sines table are small, and the angle obtained from the tables is only one of a number of possible values of which it is about the mean. Here, in order that $A+B+C$ may make 180°, we choose 83° 26′ for C.

F. C. B.

962. [A. 1. c.] *A Proof of the Binomial Theorem for a Positive Integral Index.*

1. $(a+b)^n=(n,\ 0)a^n+(n,\ 1)a^{n-1}b+\ldots+(n,\ r)a^{n-r}b^r+\ldots+(n,\ n)$ ....(A)

It is obvious that the coefficients $(n,\ 0)$, $(n,\ 1)\ldots(n,\ r)\ldots$ are integers depending solely on n and r.

By putting $b=0$ we get $a^n=(n,\ 0)a^n$. $\therefore\ (n,\ 0)=1$.

" $a=0$ " $b^n=(n,\ n)b^n$. $\therefore\ (n,\ n)=1$.

2. $(a+b)^n=b(a+b)^{n-1}+a(a+b)^{n-1}$. Expanding both sides by (A) and equating coefficients of $a^{n-1}b$, we get $(n,\ 1)=1+(n-1,\ 1)$.

Similarly $(n-1,\ 1)=1+(n-2,\ 1)$, etc.

Thus $(n,\ 1)=1+(n-1,\ 1)=1+1+(n-2,\ 1)=\ldots=1+1+\ldots$
$+$ to $n-1$ terms $+(1,\ 1)$
$=n$.

3. $(\overline{1+x}+y)^n=(1+\overline{x+y})^n$.

$\therefore\ (1+x)^n+(n,\ 1)y(1+x)^{n-1}+\ldots=1+(n,\ 1)(x+y)$
$+(n,\ 2)(x+y)^2+\ldots+(n,\ r)(x+y)^r+\ldots.$

Equating coefficients of yx^{r-1} we get

$$(n, 1)(n-1, r-1)=(n, r)(r, 1),$$

or, since $(n, 1)=n$, and $(r, 1)=r$, (see 2),

$$n(n-1, r-1)=r(n, r). \quad \therefore (n, r)=\frac{n}{r}(n-1, r-1).$$

Similarly $$(n-1, r-1)=\frac{n-1}{r-1}(n-2, r-2), \text{ etc.}$$

$$\therefore (n, r)=\frac{n}{r}\cdot\frac{n-1}{r-1}\cdot\frac{n-2}{r-2}\cdots\frac{n-r+1}{1}(n-r, 0), \text{ or, since } (n-r, 0)=1$$

$$(n, r)=\frac{n(n-1)(n-2)\ldots(n-r+1)}{1\,.\,2\,.\,3\ldots(r-1)\,.\,r}. \qquad \ldots\ldots(B)$$

By substituting in formula (A) the values thus found for the coefficients $(n, 0)$, $(n, 1)\ldots(n, r)\ldots(n, n)$, we arrive at the Binomial Theorem when n is a positive integer.

R. F. M.

963. [D. 2.] *Tannery's Theorem.*

The supposed difficulty of this valuable theorem is artificial, a very simple theorem on double limits being complicated, first by the pernicious and apparently ineradicable habit of expressing results in terms of series rather than of sequences, and then by the substitution of a childish sufficient condition for the condition of uniformity, which, though presumably not necessary, is the one which makes the conclusion obvious.

If as $n \to \infty$, the function $f(r, n)$ tends to $g(r)$, uniformly with respect to r, and if $f(n, n)$ tends to G as $n \to \infty$, then $g(r) \to G$ as $r \to \infty$.

For, given any positive value of ϵ, we can find m_1 such that $|f(n, n)-G|<\frac{1}{2}\epsilon$ for all values of n greater than m_1, and we can find m_2 such that $|f(r, n)-g(r)|<\frac{1}{2}\epsilon$ for all values of n greater than m_2 and for all values of r. It follows that if n is greater than both m_1 and m_2 and if r is equal to n, or in other words that if r is greater than both m_1 and m_2, then $|g(r)-G|<\epsilon$, and this establishes the result.

Replace $f(r, n)$ by $v_1(n)+v_2(n)+\ldots+v_r(n)$, and $g(r)$ by $w_1+w_2+\ldots+w_r$, and secure uniformity by postulating a dominating series of positive terms, and you have Tannery's theorem in its usual form, from which it must be again modified if it is to be applied to products or to any other limits except series.

It may be said that such a proof as was reproduced in the March *Gazette* (p. 72) is designed to be understood without any reference to uniformity; this is true, and the result is a devastating example of the effect of utilising a technique without exposing the ideas on which the technique is based.

E. H. N.

964. [R. 1. b.] *Kinematic Properties of a Moving Lamina.*

The theorems enunciated by Dr. C. Fox in Note 923 (*Gazette*, March, 1929, p. 351), follow very readily from the properties of the "centre of acceleration."

Using (with Dr. Fox) A to denote this point, and I the instantaneous centre at the same instant, the acceleration at any point P of the lamina has components $\omega^2 PA$ and $\dot{\omega} PA$ respectively along and perpendicular to PA, ω being the angular velocity of the lamina. The directions of the resultant acceleration of points P of the lamina are therefore inclined at a constant angle $\alpha \equiv \tan^{-1}(\dot{\omega}/\omega^2)$ to PA. But the direction of resultant velocity is perpendicular to PI. If these two directions make a constant angle λ with one another it follows that the angle API is constant, and the locus of P in the lamina is a circle—one of a coaxal system through A and I for different values of λ.

Also since it is known that AI makes an angle $\frac{1}{2}\pi - \alpha$ with the common tangent to the centrodes at I, the tangent to the circle (λ) at I makes an angle λ with this common tangent.

Moreover, the resultant velocity and the resultant acceleration are respectively proportional to PI and PA. Hence the locus of P for which the ratio of these is constant is a "circle of Apollonius," centre on AI, cutting AI in a pair of harmonic conjugates and therefore orthogonal to any circle of the previous set.

By varying the constant we get thus a second coaxal system having A and I as limiting points.

I may mention another interesting property which follows readily from the same considerations; viz. the directions of resultant acceleration of all points of the lamina which are at a given instant on a circle envelop a conic.

For if AY is the perpendicular from A to the direction of resultant acceleration of P the triangle PAY is of given species. So as the locus of P is a given circle, Y must lie on another given circle; and the envelope of PY is a conic having A as one focus and this circle as auxiliary circle.

July 5, 1929. E. H. SMART.

965. [H. 2. a.] *The Method of Parameters in solving certain types of Partial Differential Equations of the First Order.*

Under the above title, Note 932 appeared in the *Gazette* for April, 1929. One small omission may be mentioned. In the third example: $pq = x^m y^n z^l$, we are told to "put $p = x^m z^{\frac{1}{2}l}$; $q = y^n z^{\frac{1}{2}l}$." This should read: Put $p = ax^m z^{\frac{1}{2}l}$; $q = \frac{1}{a} y^n z^{\frac{1}{2}l}$, leading to the complete integral,

$$\frac{z^{1-\frac{1}{2}l}}{1-\frac{1}{2}l} = \frac{ax^{m+1}}{m+1} + \frac{y^{n+1}}{a(n+1)} + c,$$

as found by the ordinary method, *e.g.* Ex. 1 and 2 in Art. 196 of Forsyth's *Treatise on Differential Equations.* The omission of the constant a leads, of course, to the particular form of the complete integral for which $a = 1$.

Possibly a similar addition may be made to the last two lines of the above note. Instead of writing that the method can be applied whenever the differential equation can be satisfied by $p = \phi_1(x)\psi(z)$; $q = \phi_2(y)\psi(z)$, use the notation $p = \phi_1(x, a)\psi(z, a)$; $q = \phi_2(y, a)\psi(z, a)$, giving a complete integral of the form

$$F(z, a) = f_1(x, a) + f_2(y, a) + c.$$

Of course, the arbitrary constant a need not occur explicitly in all three of the functions F, f_1, f_2, but it must occur in at least one of them if the integral obtained is to be a complete integral (*i.e.* containing two arbitrary constants). Probably the use of this constant (or parameter) a in all cases would have been assumed, save for the omission noted above.

October 25th, 1929, University College, Nottingham. F. UNDERWOOD.

966. [D. 2. c.] Note on *Note* 951, p. 22, vol. xv.

The reasoning of the argument on p. 22 proves that a variable x is a constant. Let $u_x = x$; then we have

$$u_{x+1}/u_x = 1 + (1/x),$$

which gives in the limit when $x = \infty$

$$u_{x+1}/u_x = 1.$$

The solution of this equation is readily seen to be

$$u_x = a,$$

when a is an undetermined constant.

[Stirling's theorem is a little deeper—not much deeper it is true, but it is deeper—than Mr. Harvey thinks it is.] P. Q. R.

967. [V. 1. a. i.] Note on *Note* 950, p. 21, vol. xv. No. 205.

The construction given by Miss B. Naylor for inscribing the principal ellipse in a parallelogram admits of an interesting generalisation.

It may be inferred from proof (1) of the note referred to, that the construction is "projective," and that, therefore, it is applicable to any conic section. In addition to this, it enables us to construct on a given chord *any* arc of a conic.

The most general problem it solves is that of drawing (*v.* Fig. 1) an arc of an ellipse on a chord DD' of which AA' is the conjugate diameter.

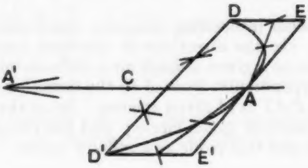


Fig. 1.

The same construction gives a parabolic arc when A' is at an infinite distance, and a hyperbolic arc when A' is on the other side of A.

Stated in this generalised form, the construction effects unification and a great saving of labour in certain parts of descriptive geometry. As an example: the finding of any section of a cone or cylinder (or of any elevation or plan of that section) is reduced to the drawing of an arc of a conic in a rectangle.

In Fig. 2, PQ is the trace of a cutting plane parallel to a generator, and the plan and true shape of the section are drawn as arcs of parabolas in rectangles.

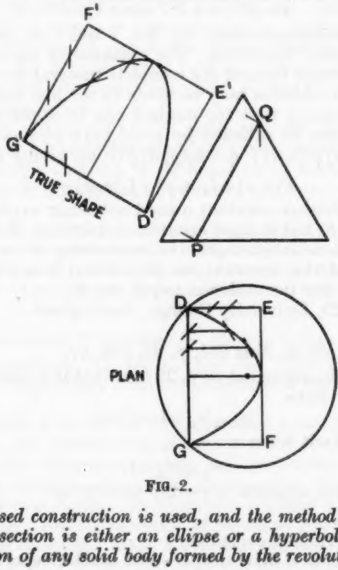


Fig. 2.

Also, *the generalised construction is used, and the method of procedure is just the same, when the section is either an ellipse or a hyperbola. Further, it may be used for the section of any solid body formed by the revolution of a conic.*

V. Naylor.

REVIEWS.

A Primer of Geometry. By W. Parkinson and A. J. Pressland. Second Edition. Pp. 304. 4s. 6d. (Humphrey Milford, Oxford University Press, 1929.)

The first edition of this text-book was reviewed in the *Gazette* in December 1923. In the present volume the original text appears with a few small additions together with some thirty additional pages of miscellaneous exercises and examination questions. The book is well arranged and beautifully printed, and the diagrams are excellent. The proof of Theorem 39, page 152, remains incomplete.

A School Geometry. By A. Walker and G. P. McNicol. Pp. viii + 492. 5s. Also published in separate parts. (Longmans, Green & Co., 1929.)

This text-book frankly returns to a sequence of propositions largely Euclidean. The first 73 pages are introductory and lead up to the formal theoretical geometry arranged in seven books, each of which ends in copious exercises. In the plane geometry the departures from Euclid are generally good, but the solid geometry is less attractive. Facing each other on pages 416, 417 are two propositions dealing with the concurrency of straight lines associated with a tetrahedron $ABCD$. One set of lines meet at O, another set at L. It does not appear that O and L are one and the same point. On page 422 a proposition is quoted which is not proved till we come to pages 433, 434. Many of the diagrams look clumsy by reason of the great thickness of the lines—sometimes approaching one millimetre. Two propositions are attributed to Appolonius, and this spelling is repeated in the Index.

A Junior Practical Geometry. By F. F. Potter and Denham Larrett. Pp. v + 138. 2s. 6d. (Pitman, 1930.)

The book consists mainly of graduated exercises in drawing and measurement, together with a few explanatory paragraphs, and summaries in clarendon type of the geometrical facts which have emerged in the different chapters, and it covers all those parts of elementary geometry which are usually taught to non-specialists in schools. The authors are conscious of a difficulty in bridging the gap between the practical and the formal work, and they attempt to cope with this difficulty by means of a last chapter entitled "The Next Step." There appears to be a tendency in these days to extend unduly the preliminary course of practical work. Is it not better to restrict the introductory work to certain definite fundamental facts which constitute a broad basis for formal deductive treatment that should not be unduly postponed ? And one cannot be too careful to distinguish the properties necessary and sufficient for the construction of a figure from others which arise in consequence and are capable of proof. For instance, if a boy learns, as on page 59, that " A quadrilateral with two equal long sides and two equal short sides, and with its angles all right angles, is called a *rectangle*," then there is placed in his path a stumbling-block which it will be very difficult to remove.

A New Geometry for High Schools. By A. A. Krishnaswami Ayyangar. Pp. xxvii + 600 + xxx pp. of Answers to Exercises. Re. 2.8. (Srinivasa Varadachari & Co., 1928.)

This text-book, prepared to meet the special needs of Indian High Schools, is the work of an enthusiastic teacher, covers in great detail the substance of Euclid I to IV and VI, and concludes with a chapter on Elementary Plane Trigonometry. There is a good chapter on Loci, which brings out clearly the double aspect of a locus. By a curious mistake on page 186 the stars " appear to move along arcs of circles from west to east."

The volume contains many interesting features, the most conspicuous of which is the wealth and variety of the exercises, many of which are original while some require considerable ingenuity for their working. Other notable features are the interesting historical anecdotes, critical notes and worked

examples, the grouping of related theorems, the discussion of fundamentals, and the explanation of the nature of geometrical reasoning.

The first chapter introduces the definitions and fundamental concepts by appeal to experience with solids, but makes no attempt to develop a broad basis of geometrical facts by appeal to intuition, so that the beginning of the course is more Euclidean in character than that now generally adopted in this country. It is, however, surprising that a book which enters so thoroughly into detail gives incomplete proofs of those theorems which establish the angle conditions for four concyclic points. W. J. D.

A History of Mathematical Notations. Vol. I, *Notations in Elementary Mathematics.* By F. Cajori. Pp. xvi + 451. $6. 1928. (Open Court Co.).

"A big book is a great evil" was an epigrammatic comment of Callimachus. A couple of volumes such as this History promises to make exhibits very vividly the extent of another evil of extraordinary magnitude, to which as time went on the great mathematicians were alive. In 1800, for instance, we find Arbogast laying down for himself the following rules as to symbols:

(*a*) *to use notations as far as possible analogous to those in use.*
(*b*) *to use none that could be replaced by those in use.*
(*c*) *to use notations as simple as possible, and yet available in every case demanded by the difference of operations.*

A few years later Laplace*, *Théorie des Probabilités, Livre* I, *Art.* I, is writing as follows about the index notation for powers, but his remarks have a general application:

"*Cette notation, en ne la considérant que comme une manière abrégée de représenter ces puissances, semble peu de chose; mais tel est l'avantage d'une langue bien faite, que ses notations les plus simples sont devenues souvent la source des théories les plus profondes; et c'est ce qui a eu lieu pour les exposants de Descartes.*"

André's volume* on *Notations* bears on the title-page the words *De quibusdam magni momenti minutiis.* Of the importance of these small things De Morgan was fully aware when he asserted that in matters of notation there is, after all, a public opinion to which appeal should be made. "Nothing is more easy than the invention of notation, and nothing of worse example and consequence than the confusion of mathematical expressions by unknown symbols *" (*Calc. of Functions*; *Encyc. Met.* Vol. II, p. 388). But on the whole he was optimistic as to the future for he strongly held the view that the symbols that survive are the fittest. It is interesting to note the suggestions of J. W. L. Glaisher (*Mess. Math.* II, p. 110) as to the notations for $1 . 2 . 3 \ldots x$, (1) $\underline{|x}$, (2) $[x]$, (3) $x!$, (4) $\Gamma(x+1)$, (5) $\Pi(x)$.

"*The first is almost peculiar to English works; it is clearly unsatisfactory, and has no chance of universal adoption; it is not even used by most of the leading English mathematicians. The second, though much the most convenient, can never become permanent, as square brackets cannot be spared from their general use in Algebra, to be appropriated to a particular meaning; the third is very popular with Continental (chiefly German) writers, but though preferable to* (1) *it is open to most of the same objections as apply to it; it would have been better if the* ! *had preceded the* x. *The fourth is universal but has the disadvantage that it involves* $x+1$ *to argument* x; *it also naturally suggests to the reader that* x *is intended to be fractional; the fifth, proposed by Gauss, has perhaps the best chance of general adoption,* $\Pi(x)$ *denoting* product to x."

Among the curiosities of which Prof. Cajori's volume is full, there are few more striking than the story of the struggle for existence in which for 400 years the minus sign was engaged. Here we have a case of what he calls "the simplest conceivable" of signs. With its companion + it is first found in print in 1489. The German "Cocker," Adam Reise, uses ÷ in 1525, but on the whole he used − more often than +. Books in 1602 and 1678 use $\div\!\!\cdot$ for "minus." In 1720 we find ÷ for "minus oder weniger." So in a Norwegian arithmetic of 1869. In a Danish scientific journal of 1915 we find a range of temperature indicated "fra +18° C. til ÷18° C.", and even so late as 1921 a Swedish journal of note has "0,72 ÷ 0,65 = 0,07."

* Dr. G. J. Lidstone kindly sent me these passages.

Well may our author say: "this study emphasizes the difficulty experienced even in ordinary arithmetic and algebra in reaching a common world-language. Centuries slip past before any marked step towards uniformity is made. It appears, indeed, as if blind chance were an uncertain guide. . . . The only hope for rapid approach of uniformity in mathematical symbolism lies in international co-operation through representative committees."

When we add that the details of the history of the minus sign fill ten pages, and that here, as throughout the work, the full name of the book referred to, its author, date, and pages of reference are all given in full, the reader will have a slight idea of the stupendous task set himself by Prof. Cajori. The second volume has not yet reached us, but we have seen enough of the quality of his achievement to realise the gratitude which is due to him from the world of mathematics.

The value of this volume is greatly enhanced by the beautiful reproduction of specimen pages, etc., from interesting documents, etc. The whole cost of the work appears to have been borne by the Open Court Company, to whose public spirit the mathematical world is under substantial obligations.

A Source Book in Mathematics. By D. E. Smith. Pp. xvii + 701. 25s. net. 1929. (McGraw-Hill Publishing Co.)

The series of *Source Books in the History of the Sciences*, of which this volume is the second to appear, is intended to "present the most significant passages from the works of the most important contributors to the major sciences during the last three or four centuries." This limitation of the periods from which the sources are drawn cuts out, as Prof. Smith laments, "most of mathematics before the invention of the calculus and of modern geometry, as well as all recent activities." But he hopes that another volume will in due course be produced, filling the *lacunae* which those who seek the sources of elementary mathematics will deplore. The difficulties of choice must have been studied with patient care by those responsible for the selections, but the succinct explanation given by Prof. Smith in his Preface will disarm the unreasonable critic.

A list of the original sources, with their subjects, in Geometry will indicate the scale on which the selection of papers has been arranged:

Desargues on Perspective Triangles and on the 4-rayed Pencil; parts of Poncelet's Introduction and Chapters I and III of his *Traité des propriétés projectives des figures*; Peaucellier's final description of his Cell; Pascal's *Essay pour les Coniques*; Brianchon's Theorem—the first part of a paper published at the age of 21 in the *J. de l'Ecole Polytechnique*; papers on Feuerbach's Theorem by Brianchon, Poncelet and Feuerbach; the first use of π for the Circle Ratio, from W. Jones's *Synopsis Palmariorum Matheseos*; Gauss, on the Division of a Circle into n equal Parts; papers by Saccheri, Lobachevsky and Bolyai on Non-Euclidean Geometry; Fermat's Introduction to Plane and Solid Loci, and the first eight pages of Descartes' *Discours*; papers by Riemann on his Surfaces, *Analysis Situs*, and the Hypotheses which lie at the Foundations of Geometry; Regiomontanus on the Law of Sines for Spherical Triangles, and on the Relations of the Parts of a Triangle; Pitiscus on the Law of Sines and Cosines, and on Bürgi's Method of Trisecting an Arc; Demoivre on his Formula; two papers on Prosthaphaeresis by Clavius; Gauss on Conformal Representation; Steiner on Quadratic Transformations between two Spaces; Cremona on Geometric Transformation of Plane Figures; and in conclusion papers on Higher Space by Cayley, Cauchy, Sylvester and Clifford, respectively.

The Committee responsible for the selection of topics and translators consisted of Professors R. C. Archibald, Florian Cajori, and L. E. Dickson—names a sufficient guarantee of the competence of those to whom the carrying out of this important undertaking was entrusted. The work done in the preparation of the material was voluntary on the part of all concerned.

The only misprint we have noticed is in l. 5 up, p. 544, where $2p$ should be 2^p, and $a^p/p!$ has lost its fraction bar.

Corrections and comments necessary in the original texts are indicated in

the footnotes. The book is enriched by many portraits and by reproductions of specimen pages and mathematical instruments.

That Prof. D. E. Smith, the "author" of this volume has been able to head such a band of assistants and to do his part in so useful an undertaking will give sincere pleasure to those who are aware of the value of his inspiration to his contemporaries.

Storia delle Matematiche. Vol. I, *Antichità—Medio Evo—Rinascimento* By Gino Loria. Pp. 497. L. 23. 1929. (Soc. Tip-Edit. Nazionale, Torino).

With the practised skill of a ready writer, Prof. Gino Loria has brought in this volume, within a comparatively narrow compass, the history of mathematics from the earliest days to the end of the fifteenth century. The introductory chapter naturally deals with the work of the earlier civilisations of Babylon and Egypt. The Babylonians had a system of weights and measures, a decimal and a sexagesimal method in arithmetic, a familiarity with large numbers (which was not attractive to the Greeks), and they were in possession of astronomical facts, and of such things as the musical, "most perfect" proportion, $12/9=8/6$, and "if $2a+b=M(7)$, then $100a+b=M(7)$." We may at any time hear of other mathematical acquisitions of this remarkable people; exploration has, indeed, brought to our knowledge one or two with which the author was apparently not acquainted when he was writing Chapter I. Additions here and to his account of the Rhind Papyrus, for the Chace-Manning-Archibald edition came out but recently, will no doubt be made at a future date.

The relations of animal and parasitic life suggest the effective title of "Greek Mathematics in Symbiosis with Philosophy" to Chapter II. Here we find traced the relations to this development associated in turn with the names of Thales, Pythagoras ("capo-stipite della nobile famiglia dei matematici"), the Eleatic Zeno, the Atomist Democritus, Antiphon and Bryson the Sophists, Plato, and Eudoxus with the School of Cyzicus.

Chapter III consists of an excellent account of the Greco-Alexandrine period and the work of the three great figures in the early progress of Geometry—Euclid, Archimedes and Apollonius. The next chapter recapitulates the completed labours of the geometers of the same period—Eratosthenes, Hypsicles, Nicomedes, Diocles, Perseus, Zenodorus, Pappus, Serenus, and the Commentators.

Astronomy and Geodesy form the subject matter of Chapter V.

The rest of the book deals in turn with Mathematics in Rome, in Europe during the Dark Ages, in China, India, and Arabia; with the Renaissance in Italy and beyond the Alps, with Geometry and Art, and finally with the early appearances of "algebra sincopata"—from Treviso to Luca Pacioli.

At the end of each chapter is a bibliography, in which, by the way, the names of English writers come off rather better than is usual in Italian works.

The author is to be congratulated on the success of his efforts to present a judicial and attractively written estimate of the leading features in the wonderful story of the early history of mathematical science, and in its subsequent development up to the Renaissance.

The Great Mathematicians. By H. W. Turnbull. Pp. viii + 128, 3s. 6d. net. School Edition, 2s. 6d. 1929. (Methuen.)

Prof. Turnbull's object in writing this unpretentious but significant little volume is adequately expressed in the Preface. It is undertaken "in the hope that it will help to reveal something of the spirit of mathematics, without unduly burdening the reader with its intricate symbolism.... I have tried to show how a mathematician thinks, how his imagination, as well as his reason, leads him to new aspects of the truth." The result is of unusual merit. Literary skill, an intense enthusiasm, a love for "the human touch," a feeling for the picturesque, and a share of the divine sense of humour have all contributed to this sketch. It will find admirers among those to whom the whole is an oft-told tale, in the upper forms of schools and among those who feel that an acquaintance with the personalities and aims, the triumphs and difficulties of those who have made a mark in this realm of thought will contribute to a general culture. Let us hear his concluding words: "There is a largeness about mathematics that transcends race and time: mathematics may humbly

help in the market-place, but it also reaches to the stars. To one, mathematics is a game (but what a game !) and to another it is the handmaiden of theology. The greatest mathematics has the simplicity and inevitableness of supreme poetry and music, standing on the borderland of all that is wonderful in Science, and all that is beautiful in Art. Mathematics transfigures the fortuitous concourse of atoms into the tracery of the finger of God."

Darstellung und Begrundung einiger neuerer Ergebnisse der Funktionentheorie. (Second Edition). By EDMUND LANDAU. Pp. 122. 9.60 R.M. 1929. (Springer, Berlin.)

The first edition of this little book appeared in 1916, at a time when it was difficult to procure German books, and it seems not to have been reviewed in the *Gazette*. The edition before me is beautifully printed (on much better paper) and, as with all Prof. Landau's books, the proofs have been read with meticulous care, so that in an attentive reading I have not noticed a single misprint, even of the most trivial kind. Practically the whole of the matter of the first edition is retained, but there are many changes in presentation, and a great deal of new matter has been added.

The book is concerned, almost entirely, with the behaviour of an analytic function as defined by its power-series,

$$f(x)=\sum_{n=0}^{\infty} a_n x^n$$

within its circle of convergence $|x|=r$ and as x tends to a point on the circle; questions of convergence of the series at points on the circle also bulk large. It is assumed that the reader is acquainted with—indeed, it is assumed that he has "at his finger ends"—the ordinary, classical theorems which are to be found in text-books on the theory of functions of a complex variable. With this equipment he is introduced to a series of theorems of the greatest interest and importance; from them I select for special mention but a few of the most interesting.

(i) Landau's own theorem that if, in the circle $|x|<1, |f(x)|\leqslant 1$, then

$$|s_n|=|a_0+a_1+a_2+\ldots+a_n|=O(\log n).$$

For fixed n the actual upper bound of $|s_n|$, for the aggregate of all functions satisfying the prescribed condition and for all points within the circle, is determined. The extension of this theorem to functions for which $|f(x)|=O(1-|x|^{-k}), k>0)$ is not given.

(ii) Hardy's theorem that, under the same conditions,

$$\mathbf{M}(r)=\Sigma\,|a_n|\,r^n=O\{(1-r)^{-\frac{1}{2}}\},\text{ as } r\to 1-0.$$

Hardy and Littlewood have given, in *Proc. Nat. Acad. Sci.* 2 (1916), 583-6,

$$f(x)=\sum_{1}^{\infty} n^{-\frac{1}{2}} e^{ian\log n} x^n \quad (a>0)$$

as an example of a function for which the difference between the orders of $\mathbf{M}(r)$ and $M(r)=\operatorname*{Max}_{|x|=r}|f(x)|$ is the greatest possible, namely $\frac{1}{2}$; here $f(x)$ is not bounded, but

$$M(r)=O\{\log 1/(1-r)\} \quad\text{and}\quad \mathbf{M}(r)\sim\sqrt{\pi}(1-r)^{-\frac{1}{2}}.$$

(iii) Bohr's theorem that, again under the same conditions, there is an absolute constant ϑ such that $\mathbf{M}(\vartheta)\leqslant 1$, together with the proof that we may take $\vartheta=1/3$, but no greater number.

(iv) The very interesting theorem of Fatou (actually stated and proved in a generalized form) that if $f(x)$ is bounded for $|x|<1$, then the Abel limit exists for almost all points of the circle $|x|=1$.

(v) The famous Hardy-Littlewood one-sided Tauberian theorem. Prof. Landau can still say, "This theorem lies very deep," but he has brought it perceptibly nearer the surface, and is now able to present the complete proof, from elementary principles, in five pages, as against the eight pages of the first edition, in which the proof followed a good deal more closely the original method of Hardy and Littlewood.

(vi) Fatou's theorem that, if $a_n \to 0$, then $\Sigma a_n x^n$ converges at every regular point of the unit circle.

(vii) Fabry's theorem on lacunary series which define a function having the circle of convergence as a natural boundary.

(viii) Polya's theorem (the truth of which was conjectured by Fatou) that there exists a sequence $\epsilon_0, \epsilon_1, \epsilon_2, \ldots, \epsilon_n, \ldots (\epsilon_n = \pm 1$ for every $n)$ such that $\Sigma \epsilon_n a_n x^n$ cannot be continued beyond its circle of convergence.

(ix) Picard's theorem that an integral function which is nowhere equal to a and nowhere equal to b $(b \neq a)$ is constant, and extensions due to Landau and Schottky, leading up to Picard's famous theorem on functions with an essential singularity, are proved on the basis of a recent theorem of Bloch (without an appeal to modular functions).

(x) Finally, in a chapter on "schlicht" functions, we have a refinement on a theorem of Koebe; it is here proved that, if $f(x)$ is "schlicht" in $|x|<1$, if $f(0)=0$ and $f'(0)=1$, then when $|x|=r<1$,

$$\frac{1-r}{(1+r)^3} \leqslant |f'(x)| \leqslant \frac{1+r}{(1-r)^3}, \quad \frac{r}{(1+r)^2} \leqslant |f(x)| \leqslant \frac{r}{(1-r)^2}.$$

It is an astonishing fact that all these difficult theorems, and many others not mentioned, should be proved in 93 pages (not including 17 pages of introductory matter—which, however, it by no means pays to skip), and proved in such a way that it is possible to follow from beginning to end without once putting pen to paper, although close attention to the proof is often necessary. Occasionally the task of the reader might have been lightened by a reference to the particular familiar theorem which was being employed—not everyone is so "gleg in the uptak" as Prof. Landau.

But it would be captious to end on a note of fault-finding. This little book is the result of an immense amount of work, the product of an intense interest in the subject; it is a model of what a book on mathematical analysis should be, in its clarity of thought and expression; and to Prof. Landau is due the sincere gratitude of all who are engaged in the cultivation of this or of some neighbouring plot of the mathematical garden.

The University, Adelaide. J. R. W.

Notions sur la Géométrie Réglée et la Théorie du Complexe Quadratique. By G. Bouligand. Pp. 84. 11 fr. 1929. (Vuibert.)

The gradual revolution in the teaching of higher geometry which is taking place in this country would be considerably helped by the existence in English of some such book as this by M. Bouligand. For, although pretty metrical properties of conics and quadrics are losing their fascination as the power and scope of projective geometry (which is able to produce these properties as trivial special cases) are becoming more widely realized, yet a full university honours' course that has time to consider any surface of order greater than two is definitely exceptional. In the same way in line geometry, the student is introduced to the six coordinates, and told something of the linear complex (because it is useful in statics !), and perhaps something of the tetrahedral complex (because it can be got as the system of normals to a family of confocal quadrics). That an attempt should be made to "squeeze into an already overcrowded syllabus" work on quadratic complexes and Kummer surfaces would seem to be unthinkable. Yet this is what M. Bouligand does, and with no sign of overcrowding.

The book is written as an appendix to the author's *Cours de Géométrie analytique* (which book, by the way, suggests at first glance an "Outlines of Everything" in fortnightly parts, but which really does cover, and cover well, a good deal of ground). It is, however, complete in itself. In it the subject of line geometry is developed practically from first principles: using homogeneous point coordinates, the coordinates of a line are defined for a line considered both as the join of two points and as the intersection of two planes, and their essential self-duality is thus emphasized. After a brief discussion of linear complexes and of lines common to two or more of them, Klein's auxiliary space of five dimensions is introduced, and is immediately used to prove the fundamental property of the quadratic complex . . .

namely, of being invariant under the correlations associated with six linear complexes which are in involution by pairs. The infinitesimal properties of general complexes and congruences are then outlined, and the results are applied to the quadratic complex, the biquadratic congruence, and their common singular locus, the Kummer surface. The careful consideration of particular cases of the complex and congruence which follows makes even clearer what is already an admirably clear and concise exposition. At the end of the book there are some examples, and a couple of completely worked out "essay questions", which, in addition to their use as exercises, serve to bring the work into closer relationship with the more familiar theory of quadrics.

If there is any quarrel with the book, it is that not sufficient use seems to be made of the Klein representation. The account it gives of the Kummer surface should be compared with that given for instance by Professor Baker in Volume IV of *Principles of Geometry*. However a five-dimensional sense is perhaps only to be acquired through the careful cultivation of a four-dimensional one, for which there is clearly no scope in a book whose object is to introduce the student to line geometry. We ought to be grateful for a book which expounds in a simple way ideas, which, if more advanced than those of metrical quadrics, are no more difficult to appreciate, and, what is an advantage in examinations, ideas which require for their reproduction intelligence rather than memory. T. G. ROOM.

Probability and its Engineering Uses. By THORNTON C. FRY. Pp. xiv+476. 30s. 1928. (Macmillan.)

It is sometimes alleged that the study of mathematics is confined in a vicious circle. Text-books are written by teachers, largely with the object of enabling their pupils to pass examinations. Those who have been most successful in these examinations, often at the cost of ignoring anything that lies outside the prescribed syllabus, become teachers themselves, write new text-books (differing from their predecessors as little as possible), and prepare a new generation of pupils for examination, and so on *ad infinitum*. Of course this allegation is exaggerated, but there is an element of truth in it. We therefore give a special welcome to a book which makes a clear break-away from tradition. Dr. Fry has no concern with examinations, and teaching is only a minor part of his duties as a member of the technical staff of the Bell Telephone Laboratories. In his preface he acknowledges indebtedness for help received from seven persons, of whom six are connected with various American telephone or telegraph companies (one was formerly an actuary), and one is an English biological statistician. It appears that there are more opportunities for a mathematician to engage in industrial work in America than in this country.

A first glance at the chapter headings shows how different is Dr. Fry's treatment from that of other books. Perhaps Chapters I-V are more or less on the usual lines, but after these we get on to Distribution Functions, Statistics, Distribution Functions in Engineering, Curve Fitting, Telephones, and Kinetic Theory of Gases. (These are not Dr. Fry's words, but *Telephones* indicates the subject-matter of Chapter X more clearly than his own—*The Theory of Probability as Applied to Problems of Congestion.*) In the examples we find information about telephones, Yale locks, psychic research, advertising campaigns, defects in screws, and the sale of dog-biscuits. The references show that the author is familiar with some Scandinavian researches which have not received much attention in England.

We shall now discuss a few special points. Probability is defined in the *a priori* way, by means of "equally likely" cases. The statistical definition is rejected as illogical, but then adopted as "an acceptable makeshift." The fallacies in many of the current treatments are clearly shown, leading to the pessimistic conclusion that logic must be ignored if we are to reach any practical applications. (The reviewer does not accept this opinion, but to explain the theory and application of what he believes to be the real solution of the difficulty would require an article in itself.) The cardinal point of Dr. Fry's "makeshift" is furnished by Bernoulli's Theorem, to which a whole

chapter is devoted. After a careful statement and proof in exact terms, the results are simplified into what is avowedly a loose statement, that in an infinity of trials the difference between the actual and the most probable *number* of successes will be infinite, while the corresponding difference for the *proportion* of successes will be zero. It is pointed out that *will be* is too strong; it is a case of extreme probability rather than certainty. The reviewer agrees with Dr. Sheppard that "the importance of this theorem is . . . mainly psychological: it lies in the fact that it was thought important. . . . This shows that the statistical basis was at the back of their minds all the time."

Dr. Fry has a chapter on the vexed subject of inverse probabilities, which is often attributed to Laplace, but really is due to an English clergyman, Bayes. The attempts to apply Bayes' Theorem have led to results which have been severely criticised. It is shown that the theorem, when properly enunciated (and it sometimes is not) is an exact one, but it has the defect that we hardly ever possess the data to make a numerical use of it. However, it is often of service when a merely qualitative probability is needed, and occasionally it enables us to make a rough quantitative estimate.

The famous St. Petersburg Paradox, which has puzzled men for generations, is dealt with very satisfactorily. The problem is to determine how much a player should pay for the privilege of tossing a penny and receiving 2^{n-1} dollars if heads appears for the first time at the nth toss. The usual answer is, that as the probability of heads so appearing is $(\frac{1}{2})^n$, the mathematical expectation is, in dollars, $\sum_1^\infty 2^{n-1}(\frac{1}{2})^n = \sum_1^\infty \frac{1}{2}$, which is infinite. To evade this conclusion, which appears repugnant to common sense, ingenious but unconvincing formulae for "moral expectation" have been devised. Dr. Fry points out that the usual solution assumes that the resources of the bank are infinite, and if we limit them to a million dollars ($2^{19} < 1{,}000{,}000 < 2^{20}$), the result becomes approximately $\sum_1^{20} 2^{n-1}(\frac{1}{2})^n + \sum_{21}^\infty 1{,}000{,}000\,(\frac{1}{2})^n = 10{\cdot}95$. Even if the bank possesses a million million dollars (possibly more than all the money in the world), the expectation would be less than 21 dollars. It might have been added that the usual solution also assumes that the game may last for all eternity.

A very good and balanced account is given of the Normal Law. As an exact law it can only be derived from distinctly artificial assumptions, and as an empirical law it is generally in conflict with observed facts. On the other hand it depends on a single variable, is easy to tabulate, and occupies what may be called a central position among Pearson's families of curves devised to fit frequency distributions. It is also useful as a first approximation to the Gram-Charlier series of curves (of which Dr. Fry gives a valuable account, giving them preference over the Pearson set). The Normal Law is therefore of great practical use as the first to which we turn in dealing with an unknown distribution, giving a rough estimate of results which can be dealt with more exactly later on. In certain cases Poisson's exponential law is useful, in which the probability of exactly r things being required is given by $e^{-\epsilon}\frac{\epsilon^r}{r!}$, where ϵ is the expectation. Dr. Fry's long and interesting account of the applications of this formula is in marked contrast to the usual inadequate treatment.

Space forbids a mention of all the good points of this book, so we will conclude by drawing attention to the feature which is certainly unique, namely, its chapter on telephones. Though this is founded on papers by Erlang, Engset, O'Dell, and others, which have appeared in technical journals, Dr. Fry appears to have improved upon his sources, presumably from his own experience. The ordinary subscriber who grumbles at the telephone service will never suspect the elaborate calculations (supplemented by forty-three pages of tables and several graphs) which are undertaken to keep down the proportion of lost calls and the average delay in getting a connection. In transforming results what the author calls routine algebra is often omitted, but it must be confessed that the reviewer found the filling in of these steps quite

as hard as many of the examples on summation of series or solution of a set of difference equations in the usual text-books. A few misprints in the final results remind us that we are dealing with probability and not with certainty.

As it is an unwritten law that unqualified praise of a book is to be avoided, we notice a tendency to make proofs and discussions unduly long. Perhaps this defect is to be ascribed to the cause responsible for the virtues of the book, namely, that the author is primarily a technical expert and not a teacher. H. T. H. PIAGGIO.

(1) **Differential Equations of Engineering Science.** By P. F. FOSTER and J. F. BAKER. Pp. vi + 182. 12s. 6d. 1929. (Ox. Univ. Press.)

(2) **Höhere Mathematik für Mathematiker, Physiker und Ingenieure.** Teil II. By R. ROTHE. Pp. 201. RM. 6.40. 1929. (Teubner, Leipzig.)

It is a long while since Prof. S. P. Thompson remarked that a man is not forbidden the use of a watch on the ground that he cannot make one himself. This, we presume, is the justification of the "Mathematics for Engineers" type of text-book. The bigoted pure mathematician replies that only a genius can create a mathematical method, but that an engineer will be a greater ornament to his profession if he understands the mathematical processes which he employs. The reviewer does not wish to add fuel to the controversy, but the two books before him suggest the question, "Do they order this matter better in Germany?" Could the *Gazette* give us an article on the subject?

The English book, specifically for the use of engineers, is an account of methods of solving simple Ordinary and Partial Differential Equations. These methods are set out clearly, and, in the more elementary sections, proofs are given. A chapter on Harmonic Analysis keeps clear of the pitfalls and so is convincing. Numerical methods are discussed, with emphasis on the Runge and Adams processes. Viewed as a collection of methods, the book seems admirable, but the aspiration of the authors, that it will give the young engineer a "knowledge of higher mathematics" and in particular an "understanding" of Partial Differential Equations, sets too high a value on the work.

Dr. Rothe's book will be of use to the mathematician as a highly concentrated text-book on Integral Calculus, Theory of Series, Determinants and Vector Calculus. The treatment is too condensed to be of value to a student attacking these subjects for the first time; once the elements are known, however, this book can be used to fill in gaps, and to extend the field of knowledge. Its exact worth to a physicist or engineer is another question altogether. Whether or no the average engineering student can be expected to appreciate, say, the doctrine of uniform convergence, is a query that cannot be discussed here. But even if he can, we fear that Dr. Rothe's style, rigorous and clear, is too meagre in its use of words to make the subject-matter fully intelligible to this hypothetical student, who would probably be content to pick out "the tricks of the trade" and leave the professional morality, in the shape of the theory, severely alone.

It would seem, then, that one of these books contains too little Mathematics, the other too much. Since the golden mean is so hard to discover, the testimony of two teachers of chemistry may not be valueless: "The trouble with our students is not that they know too much mathematics, but that they do not know enough."

Die Arithmetik in strenger Begründung. By OTTO HÖLDER. 2nd Edition. Pp. 73. RM. 3.60. 1929. (Springer, Berlin.)

In most English Universities a course of lectures on the Theory of Real Functions probably begins with a sketch of the Dedekind-Russell theory of irrational numbers, and maybe an hour is devoted to the introduction of complex numbers as ordered pairs of real numbers. But there is seldom time available for a systematic exposition of the theory of real numbers, and for this reason in particular we welcome the second edition of Prof. Hölder's little book. To it we can refer our students for the filling in of those gaps

we are compelled to create, and they will find an argument which, though often concise, is never obscure.

A short opening chapter deals with the realistic and abstract foundations of Arithmetic ; then follow chapters on Integers, Rational numbers, Irrational numbers, Negative numbers, and the problem of Measurement. Of these, the third, fifth and sixth are of very formal type, but the second and fourth, on Integers and Irrationals, will inevitably lead a thoughtful reader to ask many questions ; it is to be regretted that the copious foot-notes and references are least helpful in these very chapters. Prof. Hölder clearly does not wish his readers to allow their interests to be caught in the furious controversies now raging over the foundations of Mathematics, and endeavours to conceal the existence of such controversies. While, to the reviewer, this attitude seems mistaken, it will be judged meritorious by some. But whatever the views on this small point, there can be no doubt that the book as a whole is to be warmly recommended.

Groundwork of Calculus. By W. Hunter. Pp. 220. 5s. 6d. 1929. (University Tutorial Press.)

This book, a new member of the well-known series of mathematical text-books issued by the University Tutorial Press, contains a short first course in Calculus, logical, clear and interesting, but without any very striking features. The treatment of the primary concepts of gradients, limits and differential coefficients is at once logical and lucid. Considerable space is given to the applications of the Calculus to Mechanics and Physics. The only important defect is the lack of due proportion between the various parts. Thus, for example, $\sin x$, $\cos x$, $\tan x$, and $\operatorname{cosec} x$ are all differentiated from first principles ; we feel that space might have been saved here and given to certain sections which suffer from excessive concentration. The author tells us that the exponential function " is of great importance in Mechanics and Physics " : his account of it, pleasing as it is, should not be squeezed into the last ten pages of his book.

T. A. A. B.

Geometrische Konfigurationen. By Prof. F. Levi. Pp. 310. 24 m. and 26 m. 1929. (Hirtzel, Leipzig.)

The author of this book, starting entirely from scratch, gives an adequate and very readable introduction to two-dimensional topology and an excellent discussion of various simple geometrical figures and their groups of self-transformations. Chapter VI (on regular polyhedra) and the " discussion of the Desargues configuration," *i.e.* of everything an ingenious author can read into the Desargues configuration, are particularly good. But the book is marred by a number of misprints and minor errors, the worst of which is perhaps the fallacious proof of the theorem of relative velocities on p. 162. (And when p, q, and v keep company one is tempted to draw conclusions as to the author's handwriting !) There is a complete discussion of the Pascal figure, perhaps a trifle too detailed for a merely general interest, but very valuable to anyone with a more special interest in the matter.

To an amateur in the subject the book as a whole is exceedingly interesting : to a professional (the writer is half-and-half) the book seems rather too full in explanatory detail.

H. D. U.

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776. I gave them only an historical account of calendars, from the Egyptian down to the Gregorian, *amusing* them now and then with little episodes. . . . Many of them said, that I had made the whole very clear to them, when God knows I had not even attempted it. Lord Macclesfield, who had the greatest share in forming the bill, and who is one of the greatest Mathematicians and Astronomers in Europe, spoke afterwards with infinite knowledge, and all the clearness that so intricate a matter would admit of ; but, as his words, and his utterance, were not nearly so good as mine, the preference was most unanimously, though most unjustly, given to me.—*Lord Chesterfield*, on his introduction of a bill for reforming the Calendar. [Quoted from *The Liverpool Apollonius*, 1824, p. 82.]

CORRESPONDENCE.

To the Editor of the *Mathematical Gazette.*

Dear Sir,—Mr. Siddons' plaint about the treatment of his book by Professor Carslaw in the May issue of the *Mathematical Gazette* involves a number of problems in connection with the teaching of mathematics which, in my view, are not restricted in their importance to the school period. It raises, in fact, the whole question of the purpose and function of mathematical teaching. I am not, and except for an almost negligible period have never been, a school teacher, but although a layman in this respect I think I do appreciate the real difficulty that oppresses Mr. Siddons, and that he is challenging us to face.

To the question—what is to be the function and purpose of mathematical teaching at the school stage—there is perhaps no definite concise answer, but, broadly speaking, it might be agreed that these functions fall under three heads:

(1) to impart a knowledge of mathematical methods, their power and their limitations;

(2) to convince the pupil that in mathematics there are embodied truths in some sense which I need not define; and

(3) to accustom the pupil to appreciate such fine distinctions in logical argument *as his physical make-up will allow.*

The first is perhaps mainly utilitarian in its object, the second purely philosophic, and the third purely pedagogic. Although it does not require much experience to recognise that all three headings are interlocked, it has always appeared to me clear that nothing but confusion can arise if these three objects are not kept strictly in mind. The real difficulty of course arises from the fact that all three hares have to be chased simultaneously—no one hare can be caught without at least knowing where the other two are. In practice the stress that has to be laid on either of these objects will depend very much on the age of the pupil and his future intentions.

It might be argued that (3) and the limitations referred to in (1) cannot possibly be undertaken at all unless they are undertaken thoroughly on an absolutely unimpeachable logical basis. Whether such an "absolute" has yet been attained is a question we need not discuss here. For, after all, teachers must be realists, they have to deal with the brains that are actually in the heads of their pupils and they are therefore limited in a very definite way to logical discussion below a certain definite level; to work outside this limit is bad teaching. It suffices, I think, to remember that most of the modern exponents of rigour were themselves brought up in a less mathematically ascetic school. Euclid may now have toppled from his exalted pedestal but in my school-days he supplied just the right kind of "punch." To carry through (3) effectively it appears merely necessary then that the argument, the proofs, etc., should be as rigorous as is consistent with the brain capacity of the pupil; and to satisfy the limitations in (1) it is important that these should be accurately stated, provided the statement actually conveys something to the pupil; but the latter need not necessarily have been led through the logical proof. There is no half-way house, it seems to me, between this attitude and that of the "whole-hogger," who maintains that every proof which is presented must be fundamentally unimpeachable. If this is to mean anything it implies that until the logical basis of the "theory of number" has been laid on permanently secure foundations the teaching of arithmetic cannot be begun; that before convincing the boy that this is a house and that it may be useful for some purposes he must be led microscopically over every inch of the foundations so that he may realise that if a house *were* to be built on them there are some purposes for which it may *not* be used. This may be important to the adult, who may desire to produce a complete comprehensive logical presentation of a subject, but it is not in the least important to the school-boy.

Do not let us confuse text-books with original memoirs. A memoir extends or demarks the bounds of knowledge, it is an adjunct to research and plays its part in the development of the mature brain ; a text-book is an adjunct to teaching and presents its case to a less mature biological specimen. They have different ends in view.

If this point of view is acceptable we recognise at once how difficult is the function of a reviewer. For, in order to see the mathematical material presented in true perspective, he must be an adult in the subject, but in order to be a true critic of the presentation he must possess the mental acumen of a brilliant boy in the form for which the book is intended.—Yours faithfully,

Imperial College of Science and Technology. H. Levy.

HIGHER TRIGONOMETRY FOR SCHOOLS.

Dear Sir,—There will be little disagreement, I think, with the principles to which Mr. Siddons expresses his adherence in the May *Gazette* ; the immediate issue, which is whether he and his collaborator have succeeded in applying these principles in their work on trigonometry, is one on which readers must judge for themselves. The point I wish to raise is impersonal.

We can admit that tentative work is sometimes indispensable and often valuable and still maintain that an easy rigorous method, when one does exist, is intrinsically preferable to one dependent on delicate assumptions, however frankly these assumptions are disclosed. This is specially clear if the assumptions, or the results to which they lead, are not plausible, and here is where in the matter of the power series for the sine and cosine the case against compromise is very strong. For it is one thing to suggest that because x^n tends to zero for fractional values of x, there is some likelihood of being able to find a power series that will fit such a function as the sine over some unspecified range of small values of the argument. It is quite another thing to suggest—or as is more usual tacitly to assume—that the range over which the series fits the function has something to do with the range over which the series when discovered is itself convergent. When we consider how the relative importance of the terms of a power series changes as the variable increases indefinitely, it seems fantastically improbable that the sum of such a series can be a periodic function, and when we find that the series which, on quite reasonable assumptions, fits the sine for small values, is in fact convergent for all values, the natural conclusion surely is that the correspondence between the series and the function breaks down somewhere. That the correspondence does not break down is one of the delightful surprises of mathematics, of which the learner should not be cheated by the teacher's familiarity with the result.

The questions of the infinite products and the series of partial fractions are at present on a different footing from that of the power series. As far as I know, no proofs of these expressions have been put forward that are comparable in simplicity with the proofs of the power series by inequalities, and I agree whole-heartedly with Mr. Siddons that the substance of Prof. Carslaw's paper in the March *Gazette* is quite unsuitable for a first course. Also the morphology of the expressions reproduces so precisely that of the trigonometrical functions to which the expressions are related that the formal assumptions to which attention has to be called are really plausible. —Yours, etc. E. H. Neville.

ERRATUM.

Vol. xv, p. 129, ninth item. *For* 'commenced the building of a' *read* 'opened the'.

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